A REVIEW OF WORK DONE ON WEBER'S LAW

AS APPLIED TO THE INTENSITY OF SOUND.

Thesis presented for the degree of

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by

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INTRODUCTION.

The main purpose of this thesis is stated in the title: "A review of work done on Weber's law as applied to the intensity of sound." Section IV, in which I deal most directly with this topic is therefore the central and most important part of my thesis. It was necessary, however, to discuss, in addition, certain general considerations, and also certain other aspects of the problem of the relation between stimulus and sensation. This I have done at some length, but only so far as was necessary to establish my position, and to enable me to come to some conclusion as to the general significance and status of Weber's law. The experimental work described in Section VIII was designed more as a check on the work of previous investigators than as a positive contribution to the work on Weber's law which I have reviewed. On the other hand, a few quite new features were introduced in my experiments; these are described in their proper place. Nevertheless, throughout I have considered any experimental work more as a side-line, and therefore of secondary importance.

A few words on terminology are necessary.

1. THRESHOLDS. Except in occasional quotations I have completely discarded the use of limen. I have used the terms difference threshold and differential threshold to denote the ratio of the just noticeable difference to the absolute magnitude of a stimulus. I also use the symbolic values $\Delta R/R$, $\Delta E/E$, or $\Delta I/I$ without explanation, and (occasionally) such expressions as 'the Weber-Fechner/

Weber-Fechner ratio.' For the least perceptible stimulus value I use absolute threshold, except occasionally in such phrases as 'so many db above the threshold'. In any case, where threshold without a qualifying adjective appears, the context makes the meaning obvious.

In the references studied sensibility is not always distinguished from sensitivity. My practice is to use sensibility, usually qualified as differential sensibility, to denote the power to distinguish differences. I have preferred acuity to sensibility as denoting power to apprehend weak stimuli. When sensitivity is used, it is generally in a broad sense, covering both functions.

2. MEASURE OF FREQUENCY. Throughout I use cycles per second, or, simply, cycles, in preference to vibrations, v.d., d.v. etc.

3. PERSONS. Finding it difficult to decide on an exclusive use of either observer or subject, I have used both, normally adopting the word used in the original; in the case of foreign articles, and a few others, I have used observer to denote an active function, and subject to denote a passive function.

4. WEBER'S LAW. etc. I have avoided the expression Fechner's law entirely. Normally I use Weber-Fechner to denote a more mathematical, and Weber's law a more general treatment.

5. METHODS. I have consistently translated Minimaländerungen by 'Limits'. As regards varieties of the Constant Method, I have used 'Right and Wrong Cases' whenever it is so quoted, or when the foreign equivalent of these words is used.

Any/

Any inconsistencies that may appear are probably due to deference to the style or outlook of a given author. In a review of the type attempted rigorous uniformity of terminology is not essential, and, indeed, hardly possible.

With the exception of $\frac{\Delta R}{R}$ (and its variants) and j.n.d., I have been sparing in the use of symbols and abbreviations. These are often more of a hindrance than a help - an outstanding example is to be seen in Titchener's Experimental Psychology.

Throughout my thesis I have made only incidental mention of problems of acuity, minimum intensity required for audition, and other variants of the same subject. I have also omitted to describe purely physical or clinical methods of dealing with these problems, and this has meant passing over a great deal of work important in its own right, but not directly concerned with intensity discrimination. Accordingly, references to the work of Wien, Rayleigh, Zwaardemaker, Wegel, Kranz, and a number of other important investigators have not figured in my discussion of sound measurement. Exigencies of space and time have been responsible for these omissions, which in any case have little direct bearing on matters connected with Weber's law.

Generally speaking, I have allowed myself a fairly free hand as regards tables and diagrams. Wherever possible, I have simplified the tables given by the original authors, omitting data which I did not consider essential, and often reducing the number of significant/

significant figures. Wherever possible, too, I have replaced or supplemented a table by a diagram, since bare figures often convey very little, especially in the early work, which usually lacked measurement in absolute units. Many of the diagrams are thus my own, or partly so in virtue of adaptation. Some of these are approximate or slightly imperfect, especially as regards accurate plotting of variables not expressed in absolute units.*

In conclusion, I wish to thank all those who have assisted with advice or by giving their services as subjects in the experiments. In particular, I wish to express my deep indebtedness to Mr K.J.W. Craik, who devised and constructed the apparatus with which my most extensive experiments were performed. Mr Craik also furnished the wiring diagram and description of this apparatus, given on pp. 238-40.

* One unit of my logarithmic graph-paper contains a small fault which makes one interval slightly disproportionate. I have allowed for this in plotting points in the corresponding region; ~~which~~ in any case is practically negligible.
the fault

SYNOPSIS.

Page.

Wien's/

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VIII./

I. WEBER'S LAW AND ITS INTERPRETATION.

Few statements of a scientific principle can have undergone as complete and permanent an alteration as has taken place in the case of Weber's law. Or, to approach this fact from a different angle, it may be said that seldom has the original formulation of a principle been so obscured by further developments. Variations on this theme could be multiplied almost indefinitely, for Weber's law is probably unique in a number of ways. First of all, its author attached no outstanding significance to it; second, the name was given by another scientist, who considered it to be of almost unequalled importance in at least a very great portion of scientific knowledge; third, in applying it to cover a vast variety of phenomena, this second investigator put it in a totally different form, and it is in this form that the law is now universally known.

Re-stating the above in more specific terms, it may be said that Fechner's formulation (7) of Weber's law, a name for which he was responsible, has entirely superseded Weber's original statement. Accordingly, a great deal of confusion has arisen, both as regards terminology, and as regards the interpretation put on Weber's law or on the relationship expressed in Fechner's mathematical formulation.

Weber's original statement (270) was as follows:

"In observando discrimine rerum inter se comparatarum, non differentiam rerum, sed rationem differentiae ad magnitudinem rerum inter se comparatarum precipimus."

This is rendered by Titchener (252) as follows: "In comparing objects/

objects and observing the distinction between them, we perceive, not the difference between the objects, but the ratio of this difference to the magnitude of the objects compared."

This is a statement which does not admit of experimental verification or disproof; it is rather on a level with Bernoulli's generalization on the relation between <u>fortune morale</u> and <u>fortune physique</u>. It was probably to this aspect of Weber's law that Groselj (133) referred when he characterized it as an 'absolute principle, not impaired by any exceptions'. Objections to Weber's law in its original form must therefore be made, if they are to be made, on introspectional or even metaphysical grounds; it is doubtful, for example, with what degree of truth it can be said that we are able to <u>perceive a ratio</u>.

One must go further, however, in order to arrive at a statement of Weber's law more in agreement with what is generally understood by the term. I should imagine that a general conception of Weber's law might be stated somewhat as follows:

"In any sense-department, the smallest difference of stimulus which can be noticed is independent of the absolute value of the stimulus, but bears a fixed ratio to it." (that value) To this should be added the rider "within certain limits", since even Fechner, especially in his later work (e.g. 113) admitted this limitation. In its familiar symbolic form $\frac{\Delta R}{R} = C$, this revised statement formed the starting-point for Fechner's mathematical development of his <u>Maasformel</u> (measurement formula), $S = K \log R$.

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It will be seen that up to this point no implication of measurement of sensation has been is involved, and no statement ~~as to~~ of the relation between stimulus and sensation has been made. It is at this point, accordingly, that we may say that 'Weber' leaves off, and 'Fechner' begins. On the other hand, the expression 'Weber's law' has been extended to cover Fechner's mathematical development as well, and, in fact, this latter has received an equal share of attention from the beginning. But it is possible to argue, with Bourdon (82), that sensation and stimulus are one and the same thing, in which case any formulation of Weber's law which implies an opposition between the two must be avoided. It would, therefore, be desirable to confine the term Weber's law to the statement $\frac{\Delta R}{R} = C$, as recommended by Boring (78), reserving some such term as 'Weber-Fechner law' for the final results. The validity of this form admits of direct experimental verification, that of the latter rests on certain fundamental assumptions. A brief resumé of the mathematical development helps to make this clear. (The following abbreviated argument is adapted from Boring (78)).

By assuming that the 'Weber's law' equation

$$\frac{\Delta R}{R} = C$$

holds also for a small increment of sensation,

we get $$\Delta S = c\,\frac{\Delta R}{R}$$ - Fechner's Fundamentalformel.

Integrating (which is equivalent to summating the increments), this becomes

$$S = c \log_e R + C.$$

Since/

Since the constants in this equation are indeterminable, it is desirable to recast it in a more convenient form. If R_0 is the absolute threshold value of the stimulus, S, by definition, becomes 0; and, substituting in the last equation

$$0 = c \log_e R_0 + C.$$

Subtracting, we get

$$S - 0 = c(\log_e R - \log_e R_0)$$

$$\text{or} \quad S = c \log_e \frac{R}{R_0}$$

By a change of constant it is possible to change the base of the logarithms, so we may say

$$S = K \log_{10} \frac{R}{R_0}$$

This is the Maasformel; and if the stimulus R is measured in terms of its threshold value, we arrive at the familiar form of the Weber-Fechner law:

$$S = K \log R.$$

Fechner's assumptions, then, may be stated as follows (here I follow Brown and Thomson (88)): (i) that a sensation intensity is a measurable magnitude, and may therefore be regarded as a sum of unit intensities; (ii) that just noticeable differences (j.n.d.'s) of sensation intensity are equal at different points in the stimulus-scale, and may therefore be conveniently used as the unit intensities just mentioned; (iii) that the j.n.d. of sensation may be treated as a difference of between two sensations. These assumptions, it has often been said, are all questionable.

The/

The first of ~~these~~ assumptions raises a major problem. The balance of opinion seems was for long definitely ~~to be~~ against the measurability of sensation, and in the strictest sense I think it must be admitted that sensation is not measurable. On the other hand, a statement such as the following of Foucault (121) seems unobjectionable: "If sensation varies and decreases towards a zero-point, and does so by the shortest route (i.e., passing through the smallest number of intermediate sensations) this is pure variation of intensity." Again, Brown (86), in the 1913 symposium on Intensity differences of Sensation in the British Journal of Psychology, states that measurement of intensity - differences has been accomplished practically. Measurement of this sort has been carried out by investigators in a number of sense-departments, and the leading ~~some~~ results for ~~on~~ sound are presented given ~~later~~, in Section V. Those who use this method claim that useful, reliable, and consistent results can be obtained, and this, the pragmatic test, it can be argued, is a more valid justification than any theoretical considerations.

Claims, such as the above, for the measurability of sensation imply a belief not in the possibility of intensity of sensation, but rather of magnitude - a consciousness of 'moreness' or 'lessness' of, say, sound sensation. Such consciousness, I should say, is undoubtedly possible, and it makes little difference in what manner the magnitude is considered to have been built up, whether by sense-distances, as in Delboeuf's theory (cf. 86), by just perceptible increments as described by Fechner, or/

or in any other way. Nor do I think the criterion of divisibility of a quantity, quoted by Hicks (145) in the 1913 symposium, following Meinong's theory of intensive quality (177), affects the main issue. It is true that the light-sensation from a 30-candle power lamp cannot be divided into the light sensations from thirty separate candles, but it is just as true that an atmospheric pressure equal to that of 30 inches of mercury cannot be divided into thirty 'atmospheric-pressures' each of one inch, - and, for that matter, if one divides a pound of sugar into two half-pounds, it is the sugar that is divided, and not the pound.

This leads to what I consider the crux of the matter, namely, that sensation is neither more nor less measurable than any other phenomenon of change. This is the true answer to the 'quantity objection' that mind cannot be measured. The objection disappears, as Boring (76) says, because it applies also to the physical world. Therefore while it is true to say, with Myers(187), that sensation qua such cannot be measured, it is equally true to say that energy qua such cannot be measured, since all we have available for measurement is a spatial displacement, say of the height of a column of mercury or of the needle of a galvanometer. These examples, incidentally, drive home the pre-eminence, noted e.g. by Bonaventura (71), of visual spatial data in dealing with all other sensorial data. The only added complication in the case of sensation is that normally (unless we except such phenomena as the psychogalvanic response) no such visual 'displacement' is possible. The nearest approach, perhaps, outside laboratory/

laboratory experiments, is the extent of agitation or avoiding reaction evoked as the result of intense stimulation, e.g. by noise, although here, of course, the situation is very much complicated by interpretative processes and by the personal factor. Intensity of sensation and extent of response would both be what Myers (188) calls 'abstracted characters' of the situation in question. Nevertheless, I submit that limitations of this sort are no more serious with respect to sensation than in any other mode of measurement. Carrying the argument one stage further, we may say that a comparative judgement of 'louder' (or 'hotter', 'brighter', or whatever it may be) is made directly on the basis of experience, and not on an inferred knowledge of a difference in sound-energy, or in degree or spread of excitation in the sensory receptors. All measurement, then, is, in a sense, sensation measurement. Yet, on the other hand, some sort of distinction between stimulus and sensation, if only as different aspects of the same sequence of events, must be observed, since, as Boring (74) points out in an earlier one of his article[5], the respective modes of variation of stimulus and sensation are different. The programme of psychophysics may therefore be taken as the investigation and explanation of the difference between these modes of variation.

(6) Passing now to the second of Fechner's fundamental assumptions, it is necessary to consider how far it is justifiable to treat just noticeable differences as equal, and also how far the truth of this assumption affects sensation measurement, and, in particular, Fechner's <u>Maasformel</u> (or the 'Weber-Fechner' law:

S /

$S = K \log R$).

Fechner appears to have justified this second of his assumptions mainly by an appeal to introspection, though, as Brown and Thomson (88) point out, this observation is rather difficult to make, and in any case, results sometimes seem to prove the opposite. Johnson (153) holds, further, that the j.n.d. is determined not by introspective comparison, but by a double classification of stimuli, followed by a 'census' of the classified responses. The sensational meaning of that census is then imposed upon it my assumption. This is admittedly true of the constant method, and only a degree less so of the method of limits. On the other hand, methods of expression could be used (in which the observer adjusted the apparatus until he just perceived a difference, e.g. of loudness.) This would resemble the 'direct procedures' described by Ament (2), q.v. Such successive j.n.d.'s of sensation might or might not appear introspectively equal; with the proviso regarding difficulty in forming such a judgement noted above.

It appears that the most that can safely be said is what Lindworsky (173) describes as a very obvious assumption, namely that just noticeable differences are equal only in virtue of their being just noticeable, i.e. in possessing this defining quality in common. But this, as Watt (268) pointed out, does not make them equal increments. Again, equal differences are not necessarily equal parts of the same difference; thus, it does not follow that the tone interval g - f′ is twice the interval/

interval g - c′ because g′ - c′ and c′ - f′ are both fourths, and in that sense equal differences. Similar results have been obtained in connection with loudness, e.g., by Churcher, King, and Davies, (5) who found that one loudness was not twice another loudness because it contained twice the number of just perceptible intensity steps. Thurstone (251) also, experimenting with the method of 'equal appearing intervals' (a name which explains itself) found it wholly unsatisfactory.

The net result seems to be against the equality of j.n.d.'s. According to Hicks (145) the key to the problem lies in the fact that <u>unlikenesses</u> may remain constant while <u>differences</u> differ, - the former being presumably a mental correlate of the latter, 'difference' being used in the sense of 'real difference'. If, then, just noticeable differences cannot be assumed to be equal, Fechner's mathematical development of his <u>Maasformel</u> breaks down. On the other hand, this does not necessarily preclude the existence of the same logarithmic relationship, arrived at by another method. I return to this point later.

I do not propose to deal at any great length with the third of Fechner's principal assumptions. It is important, however, as underlying the step from the equation $\frac{\Delta R}{R} = C$ to the <u>Fundamentalformel</u> which forms the starting-point for the mathematical development. Accordingly, it too forms an essential link in Fechner's derivation of the logarithmic law, so that most of the immediately preceding discussion applies to this as well.

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The above must suffice for a general criticism of what may be termed the 'Weber-Fechner approach' to the psychophysical problem of the relation between stimulus and sensation. There remains, however, an equally important question to be decided, granted for the time being that the said Weber-Fechner approach is admissible. This second question is a matter of interpretation: what are the actual terms in the Weber-Fechner equation, or, in other words, what are the quantities between which the logarithmic relationship exists, provided that it does exist? In a discussion of this question other 'approaches' to the stimulus-sensation relationship emerge; a fuller account of these is postponed until later.

The so-called 'interpretations' are fully discussed in most text-books, and are traditionally subsumed under three heads. In each case the logarithmic relationship may be said to be located at a different point, as it were, in the stimulus-response process of which the end-product, for our purposes at least, is sensation.

Fechner's own interpretation of Weber's law was that to which the term 'psychophysical' has been applied. One of Fechner's favourite dreams, as Lindworsky (173) puts it, was to rule the whole psychological world with mathematical formulae. It was natural, then, for him to seize upon Weber's law as indicative of a mathematical (logarithmic) relation between physiological changes in the sensory centres of the cortex and the/

the corresponding sensation-intensities. In other words, it implies a direct 'psychophysical' parallelism between material and psychic events. This breaks down at once on critical examination, since the two series of events are not parallel throughout. Dodge (107) made a thorough examination of the position taken up by Fechner, and in it the following points, among others, emerge: Not all physiological changes have their corresponding psychic or conscious correlate; phenomena of consciousness may cease while certain processes of nervous metabolism still continue. Again, as Boring (76) also points out, a simple point-to-point correspondence of physiological events and sensation elements is progressively being discarded, so that a psychic element,
loudness
such as intensity, must be regarded as the resultant of what Dodge calls a physiological manifold, and accordingly some such principle as that of apperceptive integration must be called in - though merely as a descriptive, and not as an explanatory concept.

The psychophysical interpretation of Weber's law must therefore be set aside, in spite of the fact that it has the merit of being Fechner's own interpretation. Later I attempt to demonstrate that one of the alternative interpretations does not run counter to Fechner's fundamental position. Meanwhile, a few words are necessary in connection with the second, or physiological interpretation.

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This interpretation is especially deserving of attention in that it enlisted the support of such well-known 'classical' psychologists as G.E. Müller, Ebbinghaus, and James. In this case the logarithmic relationship is 'located' between physical phenomena and central nervous excitation, which is held to be directly proportional to sensation. The first part of this hypothesis now admits of verification, under certain conditions, by nerve-discharge methods, such as those used by Adrian (49).

Many experiments have been carried out in different biological fields, which show a logarithmic relationship between the two groups of phenomena stated above. Thus, Waller (265) in 1895, demonstrated a number of 'points relating to Weber's law' working with excised nerve-muscle and other preparations of the frog; e.g., the magnitude of muscle contractions was shown to vary as the logarithm of the electrical excitation applied. Radovici and Fischgold (212) obtained results similar to a 'Weber-Fechner curve' by comparing the number of rhythmic reflex contractions evoked by 'chocs faradiques', with the number of shocks applied. Results such as these, and those of other experiments, e.g., on the time of latency of reaction of Cyclops exposed to the action of ultra-violet light, have sometimes been taken, especially by French psychologists of the first quarter of this century as presenting a "strong argument for a physiological interpretation of the Weber-Fechner law" (Henri and Larguier des Bancels, (144)). Piéron (208), on the basis of these data and certain human reflex reactions, argues for a physiological/

physiological interpretation of 'Fechner's law', which he claims is true, if interpreted in this way. To this he adds the paradox that Fechner's law, which is true, is based on Weber's law, which in his opinion is indubitably false. In saying this Piéron no doubt refers to the familiar upper and lower deviations of the fraction $\frac{\Delta R}{R}$ from a constant value. If the second condition of the physiological interpretation is to hold good, these discrepancies must be explained on a purely psychological basis. Piéron suggests a number of possibilities, in the article quoted, and in a later paper (209), in which he stresses the importance of duration of stimulation.

Nevertheless, even granting the widespread incidence of such logarithmic relationships in all varieties of natural phenomena, it seems to me that to argue from these to a physiological explanation of the Weber-Fechner law involves faulty reasoning by analogy. For after all, in his measurement of sensation, Fechner's one purpose was, as Murphy (186) says, <u>to find the quantitative relation of the objective to the subjective world</u>. To me this seems to rule out the physiological interpretation conclusively, since, although it is possible to interpret 'stimulus' (<u>Reiz</u>) as referring to either actual physical energy, etc., or to the corresponding events in the nervous system, I do not think it is possible to understand by 'sensation' (<u>Empfindung</u>) anything other than the <u>consciousness</u> of such stimulation or such nervous events. And if the physiological interpretation is to hold good, the essential/

essential link is the direct proportionality between nervous processes and mental correlate mentioned above. Adrian (49) claims that the latter is a 'very close copy' of the physical events in the sensory nerves, but I do not see what evidence there can be for this other than introspective evidence. Therefore, although work of the type just discussed indicates a line of approach to the problem of 'bridging the gap' between stimulus and sensation, it appears to be a different gap from that indicated by Fechner, and certainly has nothing to do with Weber's 'observations'.

There remains to be discussed the 'psychological' interpretation of Weber's law, due originally to Wundt. The basis of the psychological interpretation is the generally admitted fact that we have no immediate knowledge of sensations in their primitive states, but only in their relations to other sensations. This amounts to the statement that we can measure only by comparing. Weber's law, in whatever form it is stated, is thus a special case of a generalized psychological law of relativity. On this view, physical phenomena (or <u>stimulus</u>), physiological processes (or <u>excitation</u>), and sensation intensity, are believed to be directly proportional to one another. The logarithmic relationship, therefore, holds between none of these, but between sensation and apperceived sensation, i.e., between the sensation and one's awareness or judgements of it.

My own opinion is that this is the only true interpretation of the Weber-Fechner law, and the only one having real psychological/

psychological significance. Orchansky (198) stated that physiologists mostly accepted the physiological interpretations, but that it was possible simultaneously to accept the psychological. With this I agree to the extent that it is possible to recognize the existence of a logarithmic relation such as those described by Waller, Henri, and the rest, so long as one does not extend the Weber-Fechner law to include such phenomena. On the other hand, the psychological interpretation is sufficient in itself to explain the psychophysical facts which Fechner (and Weber before him) studied. This view does away entirely with the difficulties of psychophysical parallelism, since it abolishes the distinction between sensation and stimulus, which, as Bourdon (82) says, are the same phenomena measured in different ways. At the same time, it represents closely enough what Fechner intended, namely, as already stated, the quantitative relation between the objective and the subjective world.

A good short resumé of the above discussion may be derived from a paper by Kiesow (158) at the Eighth International Congress of Psychology in 1926: In sensory discrimination we compare psychic contents, not nervous processes. Weber's law is therefore an apperception law, and only a psychological interpretation is possible.

This is the position I have taken up throughout this paper, in which the central portion (the review of work on Weber's law applied to sound intensity) is written from what may be called the Weber-Fechner stand-point. Other stand-points, or approaches to problems of differential sensitivity, are discussed later.

II. THE MEASUREMENT OF SOUND.

[It has been claimed (by Guernsey (13)) that Hearing provides the ideal sense-department for the study of Weber's law. The grounds for the claim were that the relative size of the just perceptible difference of sound intensity makes it possible to cover the entire hearing range in a comparatively small number of steps. While this is true, it must be admitted that a number of other conditions make sound a peculiarly difficult sense-department to deal with in a study of differential sensibility.

The difficulties fall into two classes. The first are conditioned by the physical nature of sound, in particular its lack of a simple direct unit of measurement. The second are more purely psychological considerations - i.e. peculiarities or anomalies in the perception of sound, and the inter-relation of its various attributes.

In the immediately following pages I deal with attempts to measure sound by means of a great variety of instruments and devices.] Webster, (271) who himself produced a 'complete apparatus for absolute acoustic measurements' ~~(271)~~, states the following requirements in measuring sound: (i) a source producing a continuous simple tone of known intensity; this he calls a 'phone'; (ii) an instrument for measuring in absolute units a constantly maintained sample tone - a 'phonometer'; (iii) a series of checking experiments, to be carried out in a sound-proof room, or, as in his own case, out of doors, with corrections for reflection from the earth's surface/

surface; other objects should be kept as far away as possible. How far these ideal conditions were observed is not always wholly apparent in the reports of the various investigators. In particular, the complete elimination of reflected or background noise is always a difficult task. On the other hand, perfect quiet etc. is not so absolutely essential in psychophysical as in purely physical experiments, and, indeed, it has been said, (e.g., by Foucault (120)) that silence is never complete at the surface of the earth. It may be assumed that in most cases, if not in all, experimental accuracy was as carefully maintained as possible.

Reviews of the early work on sound measurement from the psychophysical point of view are given by Titchener (252) and Pillsbury (210). Titchener's is the more complete as regards description of apparatus; Pillsbury's method is more critical and constructive. The respective dates of these sources, however are 1905 (Titchener) and 1910(Pillsbury), and one must therefore look elsewhere for an account of the work done since then. This later period contains most of the really important developments, notably that of the electronic valve, described by Kaye (155) as 'the key to the development of electrical methods of measurement.'

it is impossible to

Since I cannot here describe all the suggested methods, I confine myself in the following pages to a discussion of those having most bearing on problems of psychophysics, or those presented in a markedly psychological context. I deal more fully with the early work, since much of it was carried out under misapprehensions

later/

later avoided. Complete bibliographies up to 1929 are given in the International Critical Tables, on Sound Generators by Watson (267), and on the Detection and Measurement of Sound by Eckhardt (110).

In the early work two methods, both crude and imperfect, are conspicuous.

The first is based on the validity of the inverse square law, i.e., the principle that the intensity of a stimulus which reaches the receptor from a distant source varies inversely as the square of the distance of the source from the receptor. The inaccuracies of this method are manifold; indeed, it is virtually impossible to obtain conditions under which the law would hold absolutely for sound, since even in the open air reflection of sound-waves takes place from the ground. Fig. 1, shows actual experimental deviations from the inverse square law obtained by Churcher and King (4).

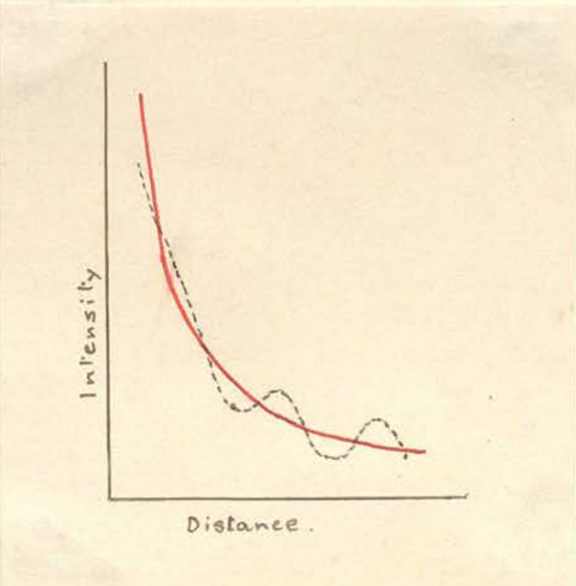


Fig 1.

Stewart (241) also notes that the relative intensities of the components of any sound will change with distance and direction from source. The best-known application is in the 'watch test", which formed the basis of Renz and Wolf's pioneer experiment (38), and which is still used by practising aurists. The method has the advantage of extreme simplicity, and is reasonably reliable accurate if rough results are sufficient for the purpose in hand.

An instrument adapted to this method is Politzer's acoumeter (see cf 252). This consisted of a steel cylinder with percussion hammer attached. Its chief drawback was the range required for its effective use.

The second fundamental method is that of falling bodies. The general principle is that the energy of a falling body is proportional to the weight of the body, and to the height and velocity of the fall. Gravity being constant, it may be said that the product of the height and the weight gives a measure of the energy. The question then arises how much of this energy is effectively transformed into sound energy or intensity. A very extensive literature exists on this subject, and conflicting results were obtained. In general, it may be said that most experimenters (though there are a few notable exceptions) found that a fractional power of the height must be taken in calculating sound intensity.

This was the finding of Schafhäutl (221), whose phonometer consisted of small balls falling from a measured height on to a vibrating horizontal glass plate. Schafhäutl held that 'sound-strength/

'sound-strength' was proportional to the weight and to the square root of the height, this being in its turn directly proportional to the simple velocity at the time of contact.

Fechner (7) on the other hand, supported a direct proportionality to height as well as weight, and in his <u>Elemente</u> published a table based on this assumption.

Vierordt (263) experimented with very weak sounds, produced by dropping lead shot on to a metal plate laid directly on a table. These had the advantage that they remained at rest immediately on falling, and also that the momentary sounds thus produced gave practically no <u>Auskling</u> (dying off of sound). The smallest balls used were obtained by partially dissolving the shot in dilute nitric acid, shaking the receptacle about in order to preserve the spherical shape.

Originally Vierordt favoured Fechner's 'greater authority' (which coincided with current physical theory), according to which a direct proportionality existed between intensity and height of fall. But 'insoluble contradictions' were encountered, and these were at first put down to subsidiary influences, such as air resistance. It was found that sounds were not equal when the products weight (p) x height (h) were constant; e.g., 2 h x p was always less than 2 p x h.

Vierordt carried out a variety of experiments within wide limits and with different plates, including some in which the subject's ear was pressed against a vertical beam mounted on the plate. All gave a relative constancy of the product

p v /

$p\,v$ ($v =$ velocity at time of impact), which is identical with $p\sqrt{h}$, and not of the product $p\,h$. For example, for a pair of weights of 56 and 36.5 mg. respectively, with heights in the ratio of about 1:2 , the sounds were judged equal for relative differences of the product $p\,v$ of .010 to .141, while the corresponding differences of the product $p\,h$ varied between .130 and .408. Vierordt therefore concluded that sound could be measured by the formula $p\sqrt{2\,g\,h}$ ($g =$ gravity), or more simply $p\sqrt{h}$.

Other contributions to this discussion are mentioned in my main review of direct Weber's law work. There is a serious fallacy underlying the whole method, namely, that intensity and loudness are at times identified, and this hopelessly confuses the issue. The trouble is partly one of terminology, especially in respect of the word <u>Schallstärke</u> which was very widely and very loosely used to cover both stimulus and sensation. More recent work has shown conclusively that equal intensities are apprehended as equal loudnesses only in exceptional circumstances, i.e., when the sounds being compared are identical in all other respects as well. Accordingly, any determination of intensity based on subjective equality of loudness is almost certain to be at fault. Even the investigators who set most store by this method nearly always had to admit the necessity of fresh empirical determinations of the fractional power of h (height of fall) in the equation $i = p\,h^{\varepsilon}$, not only for different weights and different materials, but for every pair of stimuli used.

Nevertheless, we find contemporary writers, e.g., Kämpfe (19), asserting/

asserting that sound-ratios were easily measured, - a note of optimism contrasting strangely with the statement of Baron (58), who in 1932 wrote that subjective measurement of noise, (which is what the phonometer experimenters practised) would always be laborious and disconcerting, owing to the incalculable influence of the personal factor.

Objective measurement of the noises produced by falling bodies was attempted by Oberbeck (195), who seems to have been the first to use a microphone in sound measurement; Stefanini (237) a few years later used the membrane of a telephone receiver to measure the amplitude of vibration of a tuning-fork. Oberbeck's microphone, however, was unsuitable for the measurement of the momentary noises produced by falling bodies, [and the inconstancy of his results is noted by Starke (41).]

The apparatus based on the theory of falling bodies may be divided into two main groups, according as the weight falls freely, or as the ball of a pendulum. Instruments of these two types are recommended by Wundt (46) for the study of high and low intensities respectively.

Sound-pendulums were one-armed or two-armed, and were made of a variety of materials (cf. 19). A two-armed model is shown in Fig. 2. Frequently the cross-beam and arms were heavily muffled to minimize resonance.

Instruments/

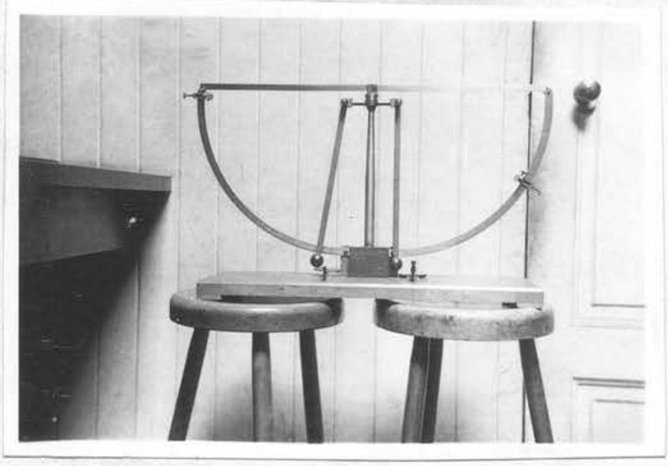


Fig. 2
Sound-pendulum.

Instruments using freely-falling bodies have a very long history. I have mentioned or described several examples elsewhere. A small instrument much favoured for laboratory purposes by Titchener was Lehmann's acoumeter (294). This may be described as a 'table model', in which lead shot were dropped from a pair of forceps resting on the head of a large screw, which could be adjusted in height. The shot dropped from a small height on to squares of glass, copper, or cardboard, which could be interchanged at will.

The/

The most familiar instrument was Wundt's fall-phonometer, and this is shown in Fig. 3. In common with all the later

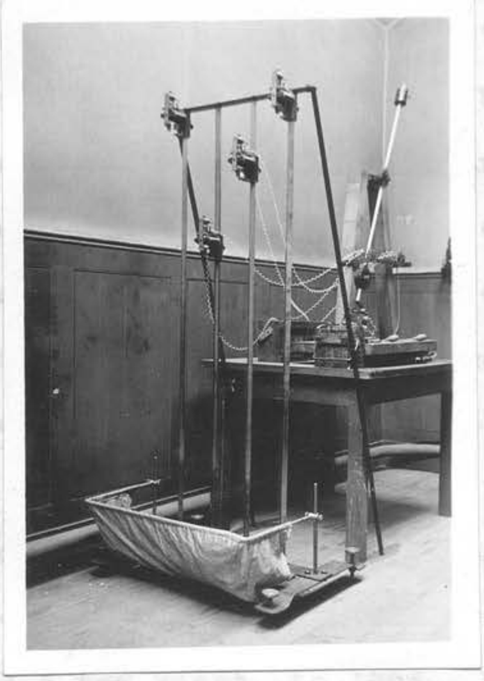


Fig. 3
Wundt's Fall-phonometer

adaptations it possessed devices for releasing the bodies electrically. The last modification I have traced was that of Bechterev (62), whose apparatus consisted of a single vertical rod with two releases, one on either side, and the necessary receptacles to ensure that no noise should occur after the actual impact. Other instruments of the same general type are described by Titchener (252). A recent refinement is the 'acouscope' of Lamboulez (165), in which ivory balls of different weight fall on the membrane of a phonendoscope, the tension of the membrane being constant. In this way 'a series of sounds which can easily be graduated is obtained'.

Pillsbury/

Pillsbury, in his review already quoted (210), deals at length with the theory of falling bodies, and with possible methods of verification, notably (i) direct measurement of the amplitude of vibration of a fork produced by the impact of a rubber ball falling from a given height, and (ii) measurement by Wien's resonator (45) of one component of a noise produced as with a fall-phonometer. But Pillsbury comes to no satisfactory conclusion, except to recommend that the use of falling bodies in sound measurement be avoided. — a piece of advice now universally accepted.

I pass now to a variety of instruments and devices less frequently used, or only suggested. In describing the earliest of these I go back a few years, to 1896. What follows is roughly in chronological order, but I have not thought it essential to preserve the sequence throughout.

Henry (17) criticized existing methods for the exploration of auditory sensitivity. Electrical sound-generators, as represented by induction-coil audiometers, were known at the time, but either these were very imperfect, or Henry was a little pessimistic as to their usefulness: ' . . ce sont là des appareils d'un maniement delicat, qui sont loin de fournir un etalon sonore rigoureusement comparable a lui-même a cause des variations d'energie de la pile '.

Henry's proposed instrument was based on an optical principle of diaphragmation, not hitherto applied to accoustics: the intensity/

intensity of a sound passing through a diaphragm is proportional to the area of the diaphragm. The apparatus consisted of a copper tube 13 cm. long, and 5 cm. in diameter with a square aperture of maximum area 4 sq. cm. One end was adapted to the ear, the other was furnished with a rubber 'hat' to contain a watch. Precautions were taken to prevent the sound arriving through the outer air. The limits of intensity available were in the ratio 1 : 13,000.

A general equation for sound sensation obtained with this instrument is noted elsewhere.

Jastrow (151) suggested a singing flame as a source suitable for the study of sound intensities. This consisted of gas burning through an aperture of about 1 mm., under a long narrow glass tube. Pitch varied inversely with the size of the tube. The amplitude of vibration could be directly observed with the aid of a mm. scale at the back of the flame, the zero point being reached when the flame was turned so low as to be just heard. A dial adjustment regulated the flow of gas, which was delivered at constant pressure. For difference thresholds, the method of flowing increments could be used; this method, however, has been shown to be theoretically unreliable, and equally so (by Bush
both theoretically and
and Austin, (92)) in practice.

A serious drawback to the use of the singing flame is that it was found difficult to make two alike. This makes the application of the standard psychophysical methods, especially that of mean/

mean error, somewhat inconvenient. A further disadvantage is that such a flame does not begin to 'sing' at once and takes time to reach its full intensity. On the other hand, in view of the advantages noted by Jastrow (amplitude directly observable, relative purity and pleasantness of tone, and possibility of delicate gradation) it is perhaps surprising that the idea has not been further developed.

Wien's apparatus (45), described elsewhere, was modified by Sharpe (226), who combined with it the optical arrangement of Michaelson's refractometer. The reflecting mirror of a refractometer was mounted on a thin glass plate, which formed part of the wall of a spherical Helmholtz resonator. The vibrations of a tuning-fork could thus be projected and photographed. This instrument was definitely suggested as suitable for testing Weber's law. At the time of writing no systematic results were available, and none seem ever to have been published, although Lewin (170) published a description of a further modification in 1922. Lewin's instrument gave an amplification of 600,000.

Instruments based on a direct application of simple c.g.s units included those proposed by Toulouse and Vaschide (254) and by Robin (293). The former consisted of drops of water, weighing .1 gm. falling 1 mm. on to an aluminium plate 10 cm. in diameter and 1 mm. thick, inclined to an angle of 20° to the horizontal, the ear being placed at a distance of 20 cm. Robin proposed/

proposed as a unit the sound produced by two halves of 1 gm. of lead meeting with the velocity due to a height of fall of 1 cm. The instrument is shown in Fig. 4.

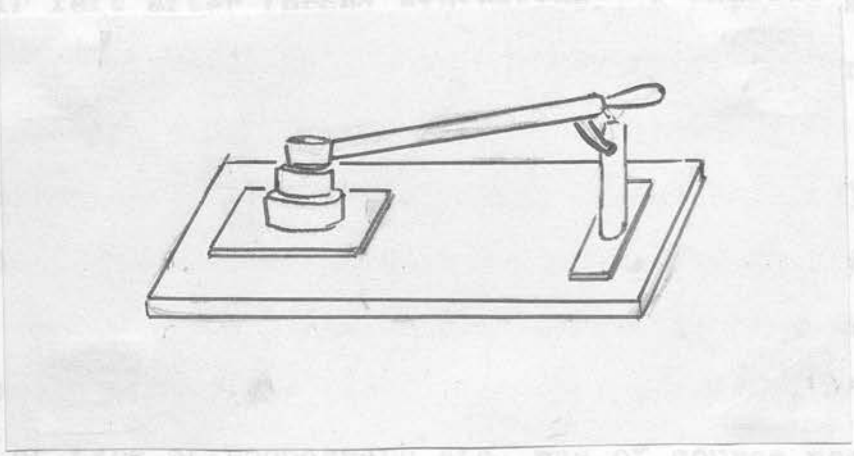


Fig. 4. Robin's acoumeter

Another simple mechanical c.g.s. measurement is mentioned by Wundt (287), who gives as the normal absolute threshold for sound, the sound produced by 1 mg. of cork falling 1 mm. on to a plate of glass, at a distance from the ear of 91 mm. This was made many years previously by Schafhäutl (221).

Speech tests have never (to my knowledge) been used in testing Weber's law. The hindrances to such a procedure are fairly obvious. On the other hand, it would be possible to make out a case for their use, on the grounds that speech is after all perhaps the most important sound of everyday life. It would be possible to use some expedient such as those suggested by a number of writers in the sphere of otology. Bezold (68) put forward a claim/

claim for the superiority of speech tests to those based on the use of watches, audiometers,telephones, and musical instruments. Equal intensity, he claimed, could be obtained by using the residual air left after forced expiration. A further step towards standardization was made by the introduction of phonographic records of speech, e.g., of the test words drawn up by Andrews(52). Instruments of this sort included those of Bentley (66) and Bryant (91). The method employed by the latter involved the repetition of the words heard, a procedure which is complicated by the introduction of the 'threshold of comprehension'. Uniformity of type of phonograph etc. was of course necessary in the use of these test-methods. The main interest throughout was anthropometric rather than psychophysical. But it might be of interest to carry through experiments on discrimination of intensity in speech (or music), perhaps using some modification of the loudness balance methods described later.

Another method used by aurists in determining auditory acuity is better adapted than speech tests to sound measurement in general. I refer to the time of damping of a tuning-fork note as a measure of the intensity of that note, or of a sound just sufficient to mask it. The time of damping may be replaced by or combined with a direct microscopic examination of the vibrations of the fork. This principle is the essence of the 'objective audiometer' described by Ostmann (200), and recommended by him as a universal standard.

A/

A large number of 'improved' tuning-forks have since been devised. In that of Bourdon (83) the sound was canalized in a rubber tube, thus avoiding the necessity of dealing with differences of distance. That of Gradenigo (130) was fitted with a pendulum which was specially arranged so as to give three standard blows of relative energy in the ratio 1 : 4 : 16, these giving initial intensities in the ratio 1: 2 : 4. A straight-line relation between energy and duration in seconds was given. The observer listened freely at a distance. Gradenigo considered this method preferable in view of experimental errors arising from the use of rubber tubing.

An automatic tuning-fork hammer specially devised for work on intensive limens was described by Bentley, Boring, and Ruckmick (67). The last-named author (220) also described in detail the theory of the accurate calculation of intensities produced by this instrument on the principle of wave-interference. It was possible to obtain any intensity from zero to a practical maximum.

Recent applications of the tuning-fork method have been made in connection with the noise measurement work, treated here separately. Davis (106) constructed a loudness scale on the basis of tuning-fork results. The fork was held as near the ear as possible, and the time of fading first to equality with a given noise, and then quite out, was noted. Since the stimulus-sensation law of the response of the ear (over a middle range) and the law of decay of the fork vibrations are both practically logarithmic, equal/

equal units of time could be taken as approximately equivalent to equal reductions of loudness.

Stowell (245) devised a tuning-fork audiometer with an automatic hammer, released by a trigger, and falling back as soon as the blow had been delivered. The instrument was so calibrated that the decay was at the rate of 2 db per second. It incorporated a short focus lens to help determine when the fork had reached its 'initial' amplitude, a stop-watch, and an interpolation scale for converting seconds of decay into decibels. A noise survey with this instrument showed it to be satisfactory, provided 3 to 5 observations of each noise could be taken and averaged, and provided that prolonged noises, up to about one minute in duration were measured.

Before dealing with two types of apparatus which have become almost universally adopted, I shall describe very briefly one or two other measurement devices.

Fig. 5 shows a <u>Hörmessapparat</u> designed by Fremel (292). The tone of an electrically maintained tuning-fork was conducted through rubber tubes, various lengths and combinations of which gave fine gradations in intensity. Fremel's chief interest was the assessment of hearing defect (as in the case of many authors here quoted), but he seems also to have tested intensity discrimination in a number of normal as well as of pathological subjects. He discovered a marked incidence of individual differences/

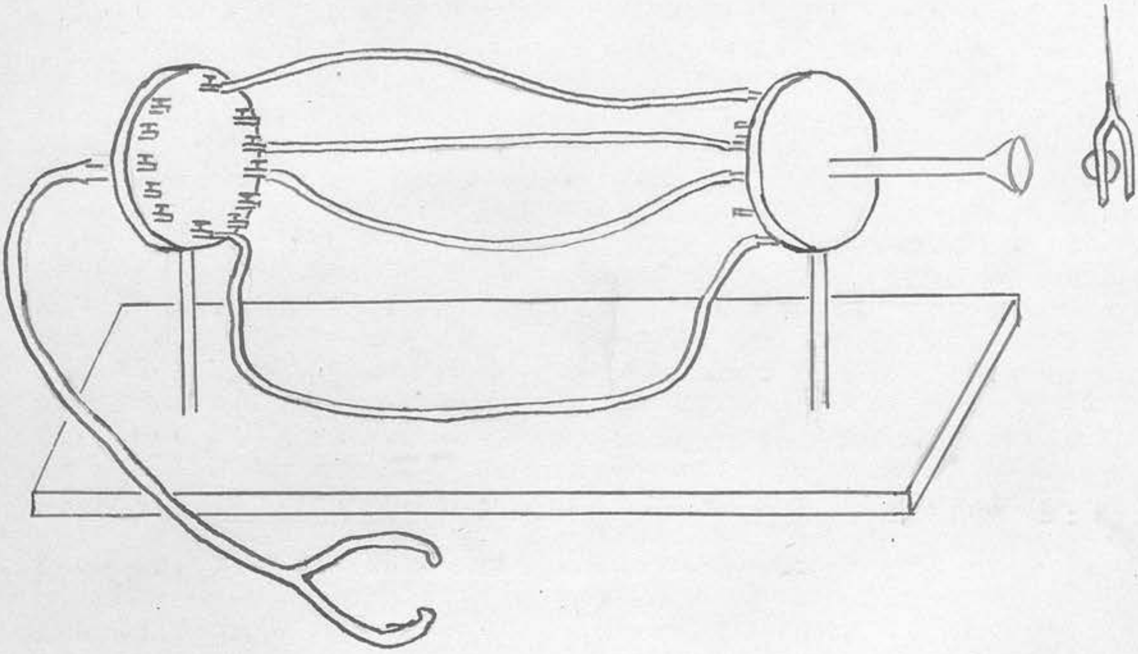


Fig. 5. Fremel's apparatus

differences; i.e., the same fine differences of intensity of the same tone were not always equally perceived by normal subjects, whether practised or not. Certainty of judgement varied greatly. Similar results were obtained with hard-of-hearing cases, - provided that they could hear the tone at all.

The variation of sound intensity with blowing pressure on tone-variators and organ-pipes was studied by Love and Dawson(175). These constitute a type of source not widely used in psycho-physical experiments, an exception being seen in the work of Macdonald and Allen (28, 1). Love and Dawson found that intensity varied/

varied directly with blowing pressure in the tone-variator, and nearly so in the organ-pipe. It was found also that only part of the energy of the air pressure was converted into sound, a result similar to those of many of the earlier investigators.

So far no mention has been made of the use of the Rayleigh disc in sound measurement. Cloud (97) described an apparatus in which a Rayleigh disc was suspended in the throat or neck of a compound resonator. The opening at the outer end could be varied by an iris diaphragm. The other end was covered with glass, through which a beam of light from a lamp was admitted. The deflection of this beam by the torque produced in the fibre from which the disc was suspended could be measured on a scale, this giving a measure of the sound intensity exciting the resonator.

Andrade (51) devised a new method of measuring sound amplitudes and intensities, consisting of smoke particles enclosed in a long tube containing air. The air is thrown into vibration by a valve-driven diaphragm. The smoke particles share the motion (with differences due to inertia and size) and, the tube being suitably illuminated the particles are drawn out into bright lines and photographed.

Most of the apparatus and methods so far discussed have had to do with either a directly measurable effect produced by sound, or with the effect produced in the human organism, as indicated by perceived loudness.

In recent times, however, it has become customary to compare an/

an unknown sound pressure with a known force produced electrically. This, as Eccles (109) pointed out in 1929, has become possible along with the invention of methods for the accurate production of sound waves smoothly variable in frequency and intensity, through and beyond the range of human audition.

Two main types of apparatus based on the principle of equivalent electrical energy have been evolved. These are described by Free (122, 123) as audiometers and acoustimeters respectively.

Free (122) quotes audiometer methods as having been in use since 1925. This, however, probably refers to their application in noise surveys, since audiometers were already known towards the end of last century. An early application to acuity determination may be seen in the work of Abraham (48). The principle common to all types is that an electrically controlled sound of some sort is produced in a telephone receiver. Intensity is governed by an attenuator. The audiometer sound is used to measure other sounds either by the principle of masking or by aural balancing, i.e., by adjusting until the audiometer sound and the sound to be measured are judged to be equally loud. (Noise measurement by aural balancing seems to have been first advocated in an article by Cohen, Aldridge and West (99).) The simplest varieties of audiometer gave a simple click, or a buzzer tone. That of Richmond (217) was designed to eliminate the electrically produced click which occurred in many models at the beginning and end of the stimulus sound and was inevitably distracting. Audiometers using phonograph or gramophone records have/

have already been mentioned. Others were supplied with tones from electrically-driven tuning-forks, e.g., that of Helverson(139).

Acoustimeters comprise practically all instruments described as noise meters or phonometers. The principle here is that sound energy is picked up from the air or elsewhere by some form of microphone. In an apparatus devised by Paris (201) the strength of a sound field was measured by the changes in resistance of a thin hot wire. Usually, however, the oscillations produced in a microphone are amplified by a vacuum-tube amplifier, the output of which is measured by a suitable meter. A network of condensers and inductances may be arranged to give direct readings in decibels above the threshold for each frequency. These instruments vary considerably, according to the frequency range they are required to cover. Portable models are described by Osbon and Oplinger (199) and by Churcher, King, and Davies (96).

The chief drawback of methods such as the above is that constancy of response of microphone and of amplifier are difficult to obtain. On the other hand, direct measurement (see F.D. Smith (233)) is effective only with fairly loud sounds. Again, sound generators vary as much in efficiency as sound meters. In particular Beat oscillators are very delicate to handle, and it is often next to impossible to eliminate hum if main's current is used.

A very reliable precision source is the thermophone, described by Arnold and Crandall (53). This requires no adjustment, the units are readily reproducible, and there is no danger of accidental detuning. The principle of operation is that electric/

electric current is converted into sound-waves by the heating effect produced by passing the current through a thin metal foil, e.g., gold-leaf. Unfortunately the low sound output limits its use to weak intensities.

The above is a brief outline of the principal apparatus adapted to the determination of absolute and differential thresholds, and other problems involving sound-measurement. There remains to be discussed, however, one important concept which has come to the fore in recent years, & has revolutionized the field of sound measurement

This most important step in the development of the theory and practice of sound measurement was the adoption of the decibel as a unit. ~~On the other hand,~~ it is impossible however to overstress the fact that the decibel is in no real sense a measure of loudness. Strictly speaking, it is not a unit of intensity either, but rather ~~although~~, as Shaxby (227) says, ~~it is~~ an exceedingly useful method of specifying intensities.

The decibel, and its practically defunct ancestor, the bel, first came to light in telephone engineering, under the name 'transmission unit' (often abbreviated TU). It originated as a unit of amplification or attenuation, the bel being simply the common logarithm of a power or energy ratio. An increase of one bel, or of ten decibels, thus represents a tenfold increase in sound energy. A further increase of ten decibels represents a second tenfold increase, or a hundredfold increase of the original intensity. A simple chart showing this relationship, extended also/

also to current measurement when the sound source is electrical, is given by Free (122), and is somewhat as follows:

Decibels:	10	20	30	40	)
Power ratio:	10	10^2	10^3	10^4	) etc.
Voltage ratio:		10		10^2	)

Reference to logarithm tables will show that, roughly speaking, an increase of 1 decibel (db) multiplies a sound intensity by 1.259, i.e., increases it by about one quarter, while an increase of 3 db doubles the intensity. These figures, and others, may be checked in a variety of ways, and I personally have found it helpful to carry out a number of such checks. Thinking in logarithmic units is a habit, perhaps a little difficult to acquire in the initial stages, but the implications of the scale can soon be mastered.

It will be seen from the preceding discussion that the decibel is nothing more than a unit of proportion, and, indeed, that there is nothing about it to confine its application exclusively to sound. It can, however, be made to have a practical and even something approaching an absolute value in sound measurement.

A decibel increase may be assigned a numerical value by the application of the formula

$$10 \log_{10} (I / I_0)$$

where I and I_0 are the respective energies or intensities of the new and original sounds. If now I_0 be given a standard or reference value it is possible to measure all other sounds by reference to that standard sound. Such a sound intensity may therefore/

therefore be taken as a starting-point or zero of the decibel scale. It is customary to take as zero a very weak intensity, approximating to the absolute threshold of hearing at some point in a middle range. Free (123) enumerates three different zeros which have been used: (i) 4.4×10^{-16} watts/sq.cm. (Noise Abatement Commission, 1929-30). (ii) 1 millibar sound pressure, equivalent at ordinary temperature and pressure to about 24.4×10^{-16} watts/ sq.cm. (iii) 10^{-16} watts /sq.cm., or .000207 bar - at N.T.P. American Standards Association (291). It is important to specify which zero is being used, since readings on the third scale are about 7 db higher than on the first, and 13.8 db higher than on the second.

If a db figure, then, is given simply as such, it must be understood that it means so many db above a given level. When the phrase '*n* decibels above the threshold' is used, a further complication arises as to which threshold, since the absolute

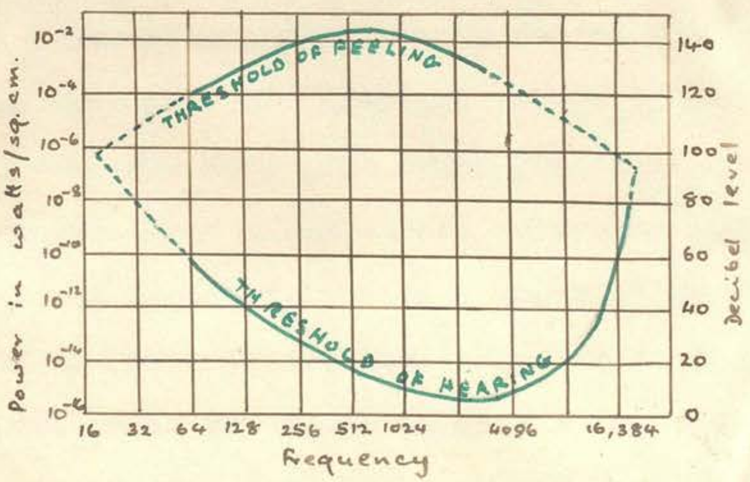


Fig 6.

absolute threshold value of different sounds varies enormously. The limits of hearing are given in Fig 6, due originally to Wegel (119).

The decibel, therefore, is not a unit of sensation. The scale is in no sense physiological, as Kaye (155) points out, and even if it was, it would not necessarily follow that it could be used as a scale of loudness. It has been claimed (e.g. by Shaxby and Gage (228)) that the decibel is approximately equal to a just noticeable difference of sound-intensity in a middle range. Baron (58) gives the figure as .3 db, adding that 1 db is required for 'useful purposes'. These figures, by the way, incidentally show a rough correspondence with the 'earlier' and 'later' values of the difference threshold ($\frac{1}{3}$ and $\frac{1}{10}$ respectively) noted elsewhere. But, as Kaye (155) shows, the j.n.d. varies from 0.2 to 9 db (or more) according to the nature of the sound. Accordingly, the usefulness of the decibel as an equivalent to the j.n.d. is probably outweighed by the danger of extending the scope of the identification beyond its legitimate limits. Such an extension produces anomalies which render the db scale useless as an indicator of numerical values of loudness. For example, Churcher and King (95) found that a sound of 90 db (above the threshold of an 800-cycle reference tone) was judged as much more than twice as loud as a sound of 45 db. A similar result, noted by Davis (106), states the situation in a striking way: very loud radio was found to have a db value of 80; very quiet radio one of 40.

Other/

Other units of measurement similar to the decibel have not found the same acceptance. Two are outstanding: the napier ($\log_e \frac{P_1}{P_o}$) and the phone ($\log_2 \frac{P_1}{P_o}$), where P_1 and P_o are sound pressures. These units are discussed by Baron (58), who gives numerical relationships between each and the decibel. Finally, it may be noted that although measurement in db has been contrasted (e.g., by Free (122)) with earlier work in c.g.s. units, it will be seen that c.g.s. units are the ultimate standard of reference, as in the case of other derived scales, suwh as those of temperature.

III. PSYCHOLOGICAL PROPERTIES OF SOUND.

with regard to actual intensity & its closest psychological correlates

Popular usage has given support to a confusion in terminology, which I have already mentioned. The sensory correlate of physical intensity is loudness, whereas the term volume is often used instead, as in the case of a 'volume' control supplied with a gramophone or radio. By 'volume' in the strict sense must be understood 'bigness' or size, or, to use a more psychological term, 'extensity'. The question whether this quality is elementary, or indeed of real occurrence in auditory sensation, has not yet been conclusively settled. Ordinary introspection suggests that a difference exists between, say, the roll of thunder and the buzz of a gnat, such as enables us to say without hesitation that the former is a 'big' sound and the latter a 'small' one, without reference to their respective loudnesses. I shall reserve judgement on this question for a little, first discussing very briefly the recent general work on tonal attributes, in which tonal volume has played a conspicuous part.

Gaetschenberger (125) speculates as to the Möglichkeit einer Quantität der Tonempfindung. This, although on the surface it would appear to be concerned with measurement of tonal sensation, deals with the possibility of tonal volume or 'fulness', corresponding to the 'extent' of the source. The author states that this possibility goes against the tacit assumption of many psychologists, but that work on the analysis of sinusoidal waves seemed to have established it. That was in 1903, and represented a fairly isolated/

isolated case, but in 1916 Rich (213) reopened the subject,
under discussion
and it has been discussed intermittently ever since.

Rich (213) gives a brief historical survey, indicating the opinions of the leading classical psychologists. Perhaps the most interesting fact is that Stumpf (246) in his later writings altered his earlier opinion, which was against the independent existence of tonal volume. His earlier view (246, vol I) was that tonal volume is merely a matter of association; others held that it is a metaphorical characterization of sensation, and not an attribute. Rich, however, carried out a series of experiments, by which he claimed to show that judgements of tonal volume could, with practice, be made with ease, and that the attribute judged was different from pitch. The difference threshold for volume was shown to be different in magnitude and course from that of pitch, and to be nearly as constant as the threshold for intensity, though this latter is rather a doubtful criterion.

In a later article (214) Rich reaffirmed his conclusions as to volume, and also discussed the suggested attributes of vocality, tonality, and brightness; the last-named was identified with pitch, and the two combined under the new name 'pitch-brightness'.

Halverson (138) studied diotic tonal volumes as a function of phase-difference, and claimed that subjects could be trained to give volumic judgements simply by practice, including the making of introspectional reports.

Boring (75) holds that volume possesses independent status, but/

but that this was long unrecognized owing to the fact that volume is a covariant of both pitch and intensity.

A fresh survey and restatement of the position was made by Gundlach (135), with special reference to theories of hearing. Gundlach and Bentley (136) a year later (1930) studied the dependence of all attributes upon phase. The general conclusion over the two articles seems to be that brightness and volume are related in a very complicated way to pitch and intensity.

Moul (183) introduced a new term, 'thickness', when he claimed to have conclusively demonstrated the existence of a multidimensional 'spread' of sound. The sounds were produced by a variety of methods, including the audio-oscillator described by Halverson (139), and forks, of which the intensity was controlled by a modification of Volkmann's sound-pendulum (7). The conclusion was based on a number of introspectional reports of the sensations experienced.

Stevens (239) put forward a claim for tonal 'density' even to the extent of a physiological basis. It was shown from the form of contours of equal density plotted against frequency and intensity, that the observers did not confuse density with either loudness or volume. In another paper (240) Stevens gives actual figures showing the percentage alteration of intensity level required in order to equate tones of different frequencies in respect of (i) volume, and (ii) density.

In addition to that of Rich (213), actual investigations of Weber's law as applied to tonal volume were carried out by Halverson (138), Gundlach and Bentley (136) and Zoll (289).

The/

The two latter investigations showed a lack of consistency of threshold values. Halverson, on the other hand, found Weber's law valid for volume with a value of $\frac{1}{4}$.

It is perhaps a little difficult to draw conclusions as regards sensory attributes which shall be uncoloured by one's own personal experience and opinion. The whole question is analogous to that of the spatial appearances of colours discussed by Katz and others. Untrained observers frequently find difficulty in distinguishing these differences in colour impressions, and similarly Zoll found that his untrained subjects could not understand what was meant by tonal volume. Personally, I must confess inability to isolate tonal volume from its covariants, and, indeed, like Vernon (262), to recognize its existence in my normal auditory experience. At present there seems to be no method of producing changes in volume while keeping frequency and intensity constant, and it is more than doubtful whether this can ever be done. That being so, any investigation of volumic thresholds must rest wholly on introspective evidence. An avenue of approach which I do not think has yet been explored would be to investigate whether tonal volume is susceptible to phenomenal regression in the same way as visual size. I should imagine that this would be most unlikely, though the phenomenon might conceivably occur with respect to loudness. Information on this point might possibly be obtained by a study of the variations of sound intensity required to produce the effect of distance in making motion pictures. I do not know whether sound directly recorded from/

from a distance can be properly interpreted as such on reproduction in a theatre, and what degree of guidance is given by visual cues. It is a matter which could be easily investigated, (if it has not already been done) e.g., by listening with eyes closed. Information concerning the relation between stimulus and sensation might be obtained as a result.

Nevertheless, it is possible that the volume attribute (or association) may have something to do with the formation of intensity judgements, especially in the case of instruments such as the phonometer when sounds produced by bodies of different size are compared, and in the case of aural balancing. The problem is one which would repay further study. Ideally, of course, it would be desirable to demonstrate the occurrence or absence of physiological correlates to all sensory attributes, but this is a field in which the barest beginnings of progress are only now being made.

No strict dividing line can be drawn between the physical and psychological properties of sound. Accordingly, what follows, although intended chiefly as a discussion of the perception of sound, and particularly of sound intensity, is also largely concerned with the physical aspect of sound. As Drever (108) says, one world of sense, and not two, is studied by both the psychologist and the physicist, and the ultimate standards must inevitably be those of the physicist. On the other hand, my own personal feeling, and it must be that of many, is that Vernon/

Vernon (261) is right when he says that the psychological properties of sounds are not susceptible of objective delimitation in physical terms, - at least with our knowledge in its present imperfect state.

One of the phenomena which lie most nearly on the dividing line is the difference in hearing according as one ear or both are used in listening. Fletcher (8) gives a table and figure for equating the two types of listening. Fig. 7 shows a smoothed curve drawn to fit the data obtained by two methods. Wide variations of judgement were found in obtaining these results.

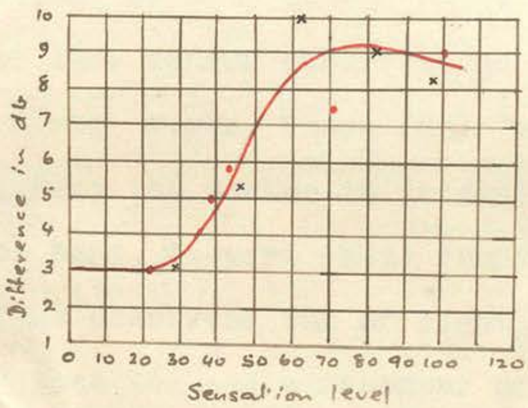


Fig. 7

Uniaural vs. binaural hearing.

More recent work by Gage (11) has shown that in the measurement of the absolute threshold for the two ears together and separately, using a pure tone of adjustable frequency, the increase of sensitivity obtained by using two ears instead of the more sensitive ear alone was about 1 decibel - hardly outside the limits of experimental error. On the other hand, improvement of both ears over the less sensitive ear averaged 5.6 decibels. These results may possibly have some bearing on the value of the difference threshold as determined by the respective modes of listening, but this question does not appear to have been investigated as yet.

An important effect of inter-relation among tonal attributes, or sensory qualities, is seen in the Broca phenomenon. As a result of more or less random observations and experiments with watch-ticks and König forks, Broca (85) found that of two sounds of the same frequency the weaker in intensity appeared higher.

On the other hand, Stewart (244) found that with tones in a middle range four observers out of eight had a constant tendency to call a weaker tone low and a stronger high, while the other four showed the reverse tendency. The tendency was marked enough to be recognized as indicating fairly prominent individual differences. Hancock (140) extended the investigation to low tones, and found that in this case a difference in intensity caused an illusion of pitch constant in direction though variable in amount; namely/

namely, that a louder sound was judged lower than a faint one of the same pitch. This agreed with Broca, but not with a general statement of Seashore (quoted in 140), based on Stewart's work, that the opposite tendency predominated, especially among untrained observers. The general conclusion drawn by Hancock was that the illusion was strong and constant with low tones, and weaker and more variable as the tone rose in pitch. The same individual, however, might show either of the opposing tendencies at different times.

The occurrence of individual differences in degree as regards the phenomenon was confirmed by Langenheck (166), who also stressed the inverse effect of greater or less intensities. With a sufficiently great difference he found that a subjective pitch-difference of half a tone could be obtained by most persons. Zurmühl (290) gave similar results, together with more definite figures as to the course of the magnitude of the phenomenon over the frequency range. Bouman (80) also studied the phenomenon, and added that a given note might become higher in pitch following fatigue from a low note, and vice versa.

Zoll (289), in a paper dealing generally with the relations of tonal attributes, showed the dependence, to some extent, of sensory intensity upon frequency. [In the intensity experiments audio-oscillators were used; that with which the quantitative results noted below were obtained was a General Radio 508-A audio-oscillator.

The results are not presented in any way which makes a conclusion with respect to Weber's law possible. However, they] are/

The results
are interesting in so far as they show how intensity may change with frequency when the sound pressure remains the same. The very marked individual differences over a small range of frequencies seems to indicate that there is more to be reckoned with here than a simple inequality of absolute thresholds at different frequencies. The consistent individual differences at 350 and 330 cycles, in particular, show the varying interpretations that may be put upon differences of pitch when judgements for intensity are being made.

Table I is to be interpreted as follows:

TABLE I.

f	Bo.	De.	El.	La.
330	L 76-24-0	L 84-8-8	E 32-68-0	S 0-5-95
335	S 0-40-60	S 4-8-88	S 0-36-64	S 5-0-95
340	L 56-40-4	? 44-20-36	E 32-56-12	? 40-0-60
345	E 0-96-4	E 8-72-20	E 0-96-4	E 10-70-20
350	S 0-48-52	S 12-20-68	E 16-76-8	L 95-0-5
355	L 88-12-0	? 52-8-40	L 52-48-0	? 60-0-40
360	L 84-16-0	L 84-16-0	E 36-48-16	L 95-0-5

Each of the frequencies in the f column was compared with a tone of 345 cycles of the same pressure, as measured by voltage. The figures show the percentage of judgements in the order <u>Louder</u>, <u>Equal</u>, <u>Softer</u>, for each of four observers at each frequency. To/

To each group of figures I have prefixed a symbol denoting the 'final' judgement. L, E, and S are self-explanatory; ? is used when the percentage of E judgements is small compared with those of L and S, but when neither L nor S shows a marked predominance over the other. As I have already said, the individual variation is very marked, though it will be seen that certain observers seem to have their peculiar tendencies (e.g., El has by far the largest, and La by far the smallest number of E judgements). However, it would appear that to some extent, at least, the general rule that higher frequencies within the lower frequency ranges tend to be more intense here holds good.

The Broca phenomenon is very important in intensity discrimination, and indeed is sometimes so strongly marked that the observer may find it impossible to judge for intensity on account of a strong qualitative difference. Often it so happens that a slight difference of this sort makes a pair of tones appear different while at the same time the observer is unable to decide whether a given tone of the pair is stronger or weaker. It is a moot point whether in judging for intensity the subject should be required to give the direction of difference, or simply to judge 'same' or 'different'. In view of these facts, therefore, it is possible to frame a case for the use of psychophysical methods in which only supraliminal differences are involved, so that there should be no doubt as to the direction of a difference.

The use in discrimination work of sounds of very low intensity/

intensity is beset with a number of special difficulties, since the perception of such tones is characterized by a high degree of inconstancy. Much work has also been done on the briefest stimulus producing a tone (summary in de Groot (132)), and at one time the problem appears to have been considered of sufficient importance for inclusion in an elementary laboratory course (see Meyer (180)).

Heinrich (143) showed that inconstancy was greater with

unpitched sounds

noises than with tones. He also investigated alterations in intensity of a watch-tick, such as are usually ascribed to fluctuations of attention, and concluded that the source of the fluctuation was not in the sound-producing process.

Bode (70) in a study of the Zeitschwelle, using electrically controlled tuning-forks, found that weak tones required a longer duration of stimulus (and therefore a greater number of vibrations) to be clearly perceived than strong tones of the same frequency. High tones needed more vibrations but a shorter time of stimulation than low tones. In this investigation, however, tones were considered equally strong when judged to be so, since there was 'no objective method of comparing intensities of different frequencies'. This I have shown elsewhere to be a questionable criterion, so Bode's results must be taken as only tentative. They were, however, confirmed by de Groot (132), who also gives definite figures and curves.

Sander (296), also using electrical tuning-forks, found that duration had a considerable effect on the intensity of the tones thus/

thus produced. This was seen in a rise at first rapid, and then more gradual, followed sometimes by a decrease. How far this is a function of the instrument is not apparent, nor is it quite plain to what intensities the phenomenon applied, but it seemed

but it is mentioned here

to be worth mentioning here, (although not quite the same pheno-

because

menon as that under discussion) in that it links up with the work of Smith and Bartlett (232) who, using forks and a buzzer, found that weak tones sometimes required as long as 4 seconds to produce their full effect. In addition, the authors demonstrated the possibility of a positive experience of silence - a debated point - on the cessation of sound which was not auditorily perceptible.

W.F. Smith (234) compared the relative quickness of visual and auditory perception, showing this to be the function of intensity. Bouman and Kucharski (81) showed that intensity required for masking was a function of duration of the masking, and further that phenomena of masking could not be reduced to processes exclusively peripheral.

[Finally,] Lifshitz (171) formulated two definite laws of sound perception relating loudness and duration. He showed that the ear integrates sound intensity (not loudness) through a definite time interval $t_2 - t_1$ so that loudness may be represented by the expression

$$L = \log \int_{t_1}^{t_2} I \, dt.$$

and that the apparent duration (D) of a sound impulse depends on its loudness:

$$D = \int_{t_1}^{t_2} \log I \, dt.$$

(l)

All the above references indicate that in auditory intensity discrimination or estimation care must be taken, especially in the case of weak sounds, to ensure that maximum intensity has been attained, as thus alone can constancy be expected. This may be taken as a further reason for avoiding work near the absolute threshold; on the other hand, if conditions are carefully controllable, it may be possible (as Smith and Bartlett found) that the most accurate results may be obtained from just this range.

I referred a page or two back to the possible effect of attention on intensity. This is a topic that has aroused a great deal of interest from the earliest times, and (many) (practically all) psychologists agree in admitting that attention tends to enhance intensity. Bruner (89) has shown that individual differences may exist in this respect, to the extent that extremely uncultivated persons, such as individuals of the primitive peoples with whom he was concerned, may show complete inability to attend to the required stimuli. A historical and theoretical review of the modification of sensation in general by attention was made (published) by Newhall (190) in 1921.

Newhall in addition conducted experiments in which a binaural noise was presented, and the subject made a judgement as to the change in location of the 'phantom' sound when the attention was directed to one component, as compared with the location when the attention was given no particular direction. On the basis/

basis of results obtained by this method, it was shown that attention had most effect on weak intensities. The evidence suggested that components from which attention was abstracted decreased in intensity rather than that the 'heard-out' component (i.e. the one to which attention was directed) became more intense.

A contrary result was obtained by Travis (255), who showed that attention to the performance of certain mental tasks decreased the absolute threshold for a tone of constant pitch. In other words, attention increased sensory intensity, but it was not necessarily attention focussed directly on the sensory field in question. A process of reciprocal innervation apparently spread to all sensory fields, thereby causing generalized heightened acuity.

Pickford (207) investigated the possibility that sensory impressions of different mode simultaneously presented might facilitate or hinder one another. Absolute thresholds of hearing were determined under ordinary conditions, and with the subject seated before a three-sided screen covered with tissue-paper of one of three colours (red-orange, ultramarine, or golden-yellow), the whole being illuminated by the same colour. It was found that, taking the threshold with no colour as unity, that with red was .99, blue 1.0, and yellow 1.15. The effects of affect, emotion, etc. were also investigated, and some slight variation was noted. When inattention was recorded, the results were found to be good. That such conditions may have some effect must therefore be admitted, but it is difficult to arrive at anything definitely conclusive.

Hartmann/

Hartmann (141) worked more specifically on the effect of strong illumination on pitch and intensity discrimination. He concluded (though he stresses the tentative nature of his results) that bright illumination facilitated intensity discrimination in particular, variability of auditory response being thus lowered under these conditions.

It is thus apparent that attention, or a complex of conditions involving changes in attention, has some effect on sensory experience. The theory underlying such phenomena is very doubtful, but Tolman (253), in showing the existence of subliminal sensory factors of which the individual is not usually conscious, claims that these are overlooked on account of more readily accessible sources of inference. This, of course, is no more than a hypothesis, but it seems to have been definitely proved that, as Tolman says, conscious attention can be directed to these very slight stimuli, and a marked improvement in their interpretation made. And much the same may be said of interpretation of one component of a complex sensory situation. For example, as Vernon (261) points out, attentive listening to any one 'figure', such as any given part, say, in a string quartet, seems to increase its apparent intensity relatively to the rest. The explanation of such phenomena is obscure, since the change occurs only subjectively.

A problem somewhat akin to the above, namely the variation of hearing at different times of the day, was studied by Bachrach(55). A limited series of experiments was carried out, using one nearly pure/

pure tone of 1175 cycles. No true sound-proof room was available, but extraneous noises were reduced to a minimum. The room was kept dark, so that time of day was the only variable. Presumably, therefore, variation was a function of the general physiological condition of the subjects as it altered during the course of the day. Individual differences and variations were observed, but the general result was that acuity was at a maximum during the late afternoon. The absolute threshold was not found to be lower during the night, a result agreeing with the observations of Kenneth and Thouless (23). Any apparent night improvement during auditory experiments, therefore, can probably be safely put down to a lowered background of noise. Owing to the limited scope of Bachrach's experiments, and to the fact that he dealt with acuity and not differential sensibility, it would be unwise to draw conclusions of either theoretical or practical significance. A contrasting result in another field was obtained by Spencer and Cohen (236) who found only a negligible correlation between fineness of visual threshold and freshness, the later being measured in different ways.

it should be noted, however, that

Reference is made elsewhere to the limits of background noise allowable (if it cannot be avoided) in work on auditory intensity. The question of hearing in the presence of a secondary sound is one that has attracted attention for well over two hundred years. At one time it was generally believed that under certain conditions noise improved hearing, especially in deaf persons. Beatty (61) holds that in most cases the effect is spurious/

spurious, but that rare cases occur in which acuity is enhanced by noise, by means of a process which has yet to be explained. Again, an anonymous writer (54) in Science, 1925, makes the following statement:

"It seems reasonable to suppose that heavy vibrations occurring in force and in more or less regular succession, or practical continuity tend to jar the stiffened transmission mechanism into a vibration or state of sensitivity of its own, when it is able to pick up and conduct those lesser vibrations of a higher pitch, which alone would be quite incapable of activating it."

Quantitative work, however, does not lead to any such conclusion. Wever and Truman (281) investigated the course of the auditory threshold in the presence of a tonal background, giving a detailed account of the sensory experiences of their subjects, especially in terms of Gestalt figure-ground relationships. It was found that the threshold in terms of a particular tone was considerably raised at the introduction of a second constant tone, and that under given conditions it might begin immediately to decline and approach normal. Work by Gage (11) on differential thresholds under somewhat similar conditions is described elsewhere.

A full discussion of the underlying theory is not necessary
the present
for my purpose, especially the theory relating to alleged improvement of hearing in the deaf. Explanations are suggested by Wever (277), Knudsen (161) and Beatty (61). Knudsen also investigated (160) the comparative interference efforts of noises and tones. The same author and Jones (162) showed that subaudible vibrations had no facilitating effect on hearing.

Modifications of intensity may arise through effects of fatigue. Opinions on this point have varied considerably. Extremes are to be seen in statements by Myers (187), who described auditory sensations as having no refractory period and virtually no fatiguability, and by Kellogg (22), who found fatigue effects even in experimental periods of 15 to 20 minutes. Oddly enough, Fletcher, in his 'Speech and Hearing', (8) makes no direct reference to the topic. Beatty (61), however, deals with it briefly, giving references mostly to fairly recent work. The reliability of the results of the earlier work is mentioned by Rawdon Smith (230).

The last-named author defines Auditory Fatigue as follows: ' .. that decrease in sensitivity which follows on continuous stimulation of the hearing mechanism, manifesting itself as a decreased response to a stimulus of constant physical intensity'. This is what Pattie (203) calls 'intensity fatigue'. With his 'localization fatigue' I am not here concerned. In an earlier paper (202), Pattie showed that the fatigue-effect was not specific with respect to frequency, since intensities of tones differing in pitch from the fatiguing (or 'stimulating') tone were decreased by stimulation with the latter. Both authors quoted give rough numerical results. Pattie (202) states that the apparent decrease in intensity is less than 'one limen'. On the other hand, Rawdon Smith shows that under certain conditions an absolute threshold may be raised by as much as 60 decibels.

The/

The most recent work is that of Ewing and Littler (112). Their chief conclusions may be stated as follows: As a result of intense stimulation, auditory acuity may be diminished to an extent which depends upon duration, intensity, and frequency of the stimulating sound. Individual ears vary greatly in manner and extent of their reactions to intense sound, but the behaviour of a given ear is predictable (within limits) on the basis of previous observation.

nt
(k)

Further peculiarities or anomalies of auditory sensation come to light from time to time. An interesting case is the following:

A difference of differential threshold for slow and rapid intensity changes respectively was demonstrated by Rawdon Smith and Grindley (231). It was experimentally shown that if the intensity of a sound oscillates in a 'saw-tooth' manner, sudden changes may be perceptible when gradual changes are not. It was found possible to produce the illusion of progressive increase in loudness while the intensity-level remained unchanged, a record of the judgements being obtained by the use of a kymograph.

The above outline indicates some of the psychological characteristics of sound sensation to be taken into account in determinations of differential sensibility. Some of the results obtained refer more specifically to auditory acuity, but in most cases sensibility might conceivably also be affected. On the other hand, as I think I have said elsewhere, it is impossible to argue/

argue from one set of conditions to another not entirely analogous.

Two further points remain to be mentioned. First, there is the question of the 'time-errors' originally noted by Fechner. Following Fechner, it is usual to give the name positive time-error to a tendency to underestimate the second of a pair of stimuli, and that of negative time-error to the opposite tendency, i.e., over-estimation of the second stimulus. Fechner noted the predominance of the negative time-error in many sense-departments, and in order to account for it advanced the famous 'memory-image' theory, according to which we have in successive comparison, only a 'faded' memory-image of the first stimulus with which to compare the second. In audition, therefore, the second of a pair of equal stimuli tends to appear subjectively louder than the first. An increase of interval between stimuli might thus be expected to affect the difference threshold. Results bearing on this obtained by Knudsen (25) are noted elsewhere.

It is doubtful how far it is possible to come to any conclusion on this matter. As far back as 1898 Schumann claimed to have disposed of the memory-fading theory. A full discussion of the time-error is given by Needham (189), who shows that a single explanation should be possible which should account for both positive and negative time-errors. The same author notes a tendency in modern psychophysical work either to ignore the error, or simply to recognize it with attempting to eliminate it. Culler (104), on the other hand, holds that different time and space/

space orders in the presentation of stimuli yield essentially different thresholds, all of which must be determined and averaged to arrive at a true result.

Second, in any standard psychophysical method, especially that of Right and Wrong cases, there is a tendency (sometimes quite unconscious), to make use of 'back comparisons', i.e. to compare a variable stimulus with a previous or an 'established' variable instead of or as well as with the standard. Thus Rubin (219) concluded that an auditory phenomenon depended not only on the time position and qualitative and quantitative properties of its central stimulus, but also on those of the preceding and following stimuli. Similarly Onoshima (197) proved according to Gestalt theory that judgement of pairs of comparison tones on a criterion of intensity depended on the rhythmical Gestalt of the whole series.

kk

In the two preceding sections I have not found it possible, nor considered it essential to draw a hard and fast distinction between the objective and the subjective qualities of sound. This is in keeping with the tendency no longer to differentiate rigidly between stimulus and sensation. Troland (256) noted a gradual relegation in modern scientific thought of sensory qualities from physics to psychology. The surrogate physical conceptions, to quote Troland further, are for the psychologist the stimuli corresponding to the respective qualities, when the latter are regarded as sensations. But since the stimuli act not immediately on consciousness, but on the physiological organism, consciousness may at least in part be represented as a mathematical function of certain aspects of organic structure and activity. To my mind this seems to justify some sort of mental or sensational measurement, irrespective of the validity of the position built up by Fechner on the basis of Weber's law.

[The main section of my paper follows, i.e., the part most directly describable as 'a review of work on Weber's law applied to the intensity of sound.']

IV. WEBER'S LAW APPLIED TO THE INTENSITY OF SOUND.

At the time of the publication of the Elemente (7) two researches were available.

The first of these was published in 1856, and was the work of two medical undergraduates at Tübingen - T. Renz and A. Wolf (38). Their experiments, carried out at the suggestion of Vierordt, are described as "über die Untershheidung differenter Schallstärken." The authors note that experimental results were available on pitch-discrimination, but none on intensity-discrimination. They add that they would have preferred to use tones, the intensity of which could have been measured objectively, and conjecture that difference-sensibility for tones would be greater than for noises, since tones are the more "elementary". The difficulties, however, proved insurmountable, and a watch was used as source of sound.

The general experimental method used depended on the validity of the inverse square law. According to the authors, the deviations due to the non-punctiform nature of the source were small enough to be neglected. The apparatus consisted in essence of a horizontal grooved wooden board, with a smaller vertical board at one end. In this latter a hole was cut to admit the subject's ear. Threads crossed over this hole gave proper alignment, and a mirror device ensured a constant position of the subject's eyes, and so of his head. In the groove a wooden slide was arranged; on this the watch was placed. A cardboard disc was used to screen the sound when necessary, and the whole apparatus was padded, to minimize extraneous sounds. Their complete elimination, however, was found to be impossible/

impossible, and this, together with day-to-day variations of the absolute threshold, made work at very low intensities out of the question. Accordingly, it was not possible to obtain absolute measurement of the sound, in terms of the absolute threshold as unit. Scale readings gave distances of the watch from the mouth of the outer acoustic passage, and these were converted into "Schallgrössen."

Only one intensity seemed to have been used as standard, and this is given a stimulus-value of 1000. The threshold investigated was a lower threshold, both time-orders being used. The numerical results are given in three tables. In (Renz and Wolf's) Tables II and III values for points close together are combined; Table III averages the time-orders. The authors themselves state their results as follows: Certainty of judgement grows with increasing difference of intensities. Sounds in the ratio 100 : 72 are clearly distinguished under all circumstances. When the ratio is 100 : 92 the number of Right judgements is slightly greater than that of Wrong and Doubtful judgements. A good agreement between the two observers was obtained, except in respect of one "reversal" in the case of Wolf, who is described as not musical. Wolf discriminated more keenly when the stronger sound came first, Renz when the weaker came first.

Part of the author's Table III is reproduced below, together with/

with extra columns giving percentage of Right + ½ equal cases.

TABLE II.

Relative intensities	Wolf.		Renz.	
	Right	R + ½ eq.	Right	R + ½ eq.
1000 : 919.5	56.5	71.2	53.3	67.7
1000 : 848	84.6	93.5	85.6	89.0
1000 : 778	81.1	85.2	97.2	98.1
1000 : 716	100	100	100	100

I have also given the percentages of 'Rights' in Fig. 8. It will be noticed that neither curve reaches the 50% mark. Owing to the very questionable accuracy of the results I have not considered it necessary to calculate thresholds using either the 50%

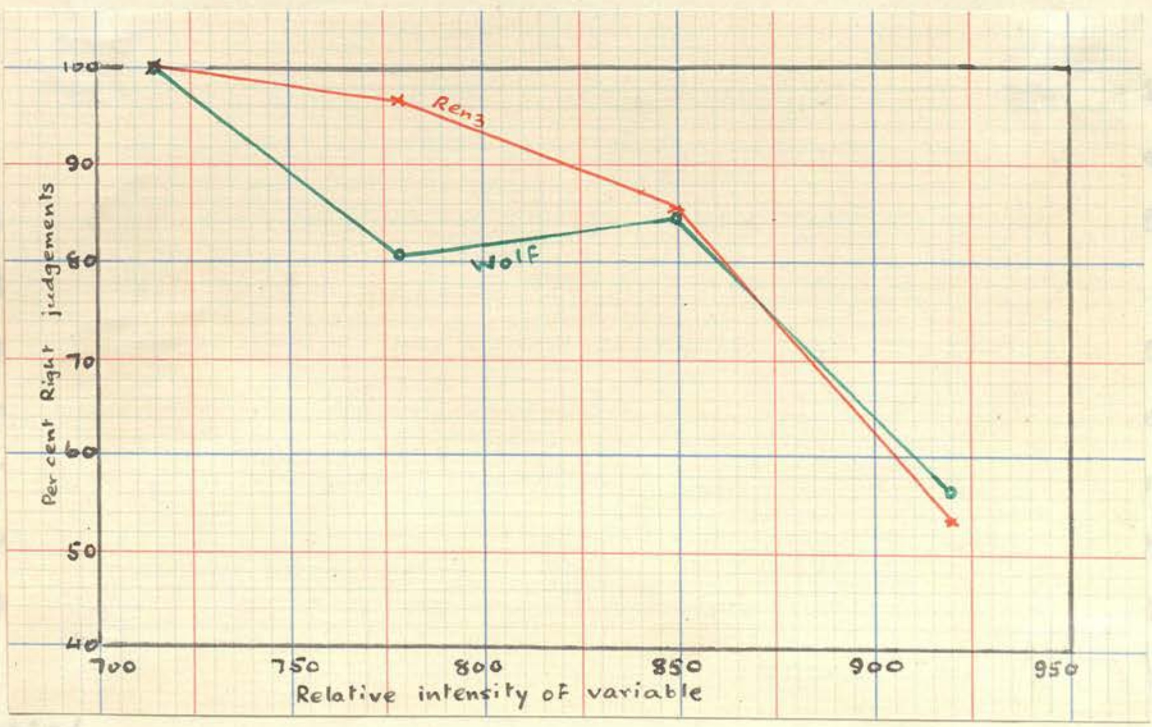


Fig 8.

or the 75% (see 124) criterion. The main interest of this investigation lies in its apparent anticipation of the standardization of psychophysical methods.

The second research mentioned by Fechner was that of Volkmann (7, pp.176-8). Volkmann carried out two series of experiments. In the first he used a simple improvised sound-pendulum, with a strong knitting-needle as axis, and a wooden hammer, striking against a four-sided glass flask. Two heights were found, such that the observer could always be certain which gave the stronger sound, but sufficiently close to make the observer judge sometimes wrong and sometimes right. Observations were taken at four distances - $1\frac{1}{2}$, 6, 12 and 18 paces, and it was found that judgement remained as sure and correct at all distances. From this it was concluded that the difference threshold was independent of the absolute value of the stimulus.

Volkmann's second group of experiments was carried out with a fall-apparatus consisting of a single graduated prismatic beam, with two holders indicating height. From these, steel balls of equal weight were dropped by hand on to a steel plate. Exact values are not given, but Volkmann states that the distance of the observer from the instrument varied from 1 to 6 meters; the weight of the balls from 1.35 to 14.85 gm.; and the heights varied in the ratio 3 : 11. Numerous experiments within the range of these limits gave, for two subjects out of three, a ratio of intensities 3 : 4 for which a difference could be accurately judged. With a ratio/

ratio of 6 : 7 considerable uncertainty occurred. It is interesting to notice, however, that for Fechner himself as observer the ratio 3 : 4 produced frequent wrong judgements. The general conclusion was that a difference threshold of about $\frac{1}{3}$ had been established, and that this result agreed well with the findings of Renz and Wolf.

No further work on the difference threshold for sound was done for nearly twenty years. Indeed, Fechner (113) in his <u>In Sachen</u> (1877), and G.E. Müller (184) in the <u>Grundlegung</u> (1878) mention only Volkmann's experiments, and even these as imperfect.

In 1879, however, Nörr (34) made an investigation in which he introduced a number of refinements as compared with Volkmann's experiments. Two instruments were used instead of one, and precautions were taken so that the balls should fall on points of the iron plate as near to each other as possible. The author himself acted as subject, and every effort was made to keep conditions constant. Catch-experiments were introduced, in which identical sounds were presented. (I have given the results of these expressed as a percentage in Table III.) Seven intensities were used, each with three different comparison stimuli. Units of intensity were obtained by multiplying weight of ball, height of fall, and velocity of fall. Table III gives simplified data concerning the relative intensities/

TABLE III.

Wt. of ball. mg.	Ht. of fall of standard mm.	Intensity. absolute	Intensity. relative.	Catch experiments. No.	First greater	Second greater	Equal.
6.7	7.5	c.2500	1	118	24.6	28	47.4
26	3.8	c.7000	2.7	71	22.5	21.1	56.4
146	6.35	c 5×10^4	20.7	122	15.6	23.7	60.7
441.5	22.6	c 3×10^5	114.3	109	21.1	33	45.9
c. 10^3	10.1	c 4×10^6	1712.1	104	14.4	22.1	63.5
c. 10^4	10.2	c 4×10^7	17035.8	81	9.9	22.2	67.9
c. 10^5	30	c 8×10^8	305563	102	28.4	15.7	55.9

intensities, etc.; Table IV gives the actual experimental results. in abbreviated form)

It will be seen from Table III that in all cases except two the time-error is in a negative direction, i.e., it tends to produce an over-estimation of the second stimulus.

TABLE IV.

Relative intensity of standard	Per cent. Difference of variable.	No. of trials.	Percentage of Judgements.	
			Rt.	Rt. + ½ eq.
1	4.42	389	75.9	79.77
	8.85	351	83.2	85.05
	17.4	180	90.0	91.95
2.7	5.01	328	83.2	86.1
	9.89	317	89.6	91.7
	19.04	108	92.65	94.95
20.7	4.9	386	80.05	84.37
	9.56	398	84.45	89.02
	18.58	160	92.35	95.17
114.3	5.1	338	67.8	74.75
	9.6	368	82.05	85.77
	18.8	143	90.2	94.4
1712.1	4.9	357	72.6	78.75
	9.4	280	84.5	89.12
	18.2	119	91.95	94.35
17035.8	5	403	80.3	84.55
	10	389	91.75	94.07
	20	152	97.3	97.97
305563	5	447	79.35	82.82
	10	381	89.55	92.27
	20	186	97.35	98.15

Nörr gives no threshold values derived from these figures, and, indeed, it is very difficult to find a statistical procedure which will/

will deal with them satisfactorily. The most obvious course is to use extrapolation, on the basis of the Rt + ½ eq. figures, and taking 75% as the proportion yielding the threshold value. But an inspection of the curves in Fig 9 suggests that in most cases the 75% point would fall below the value of the standard, which

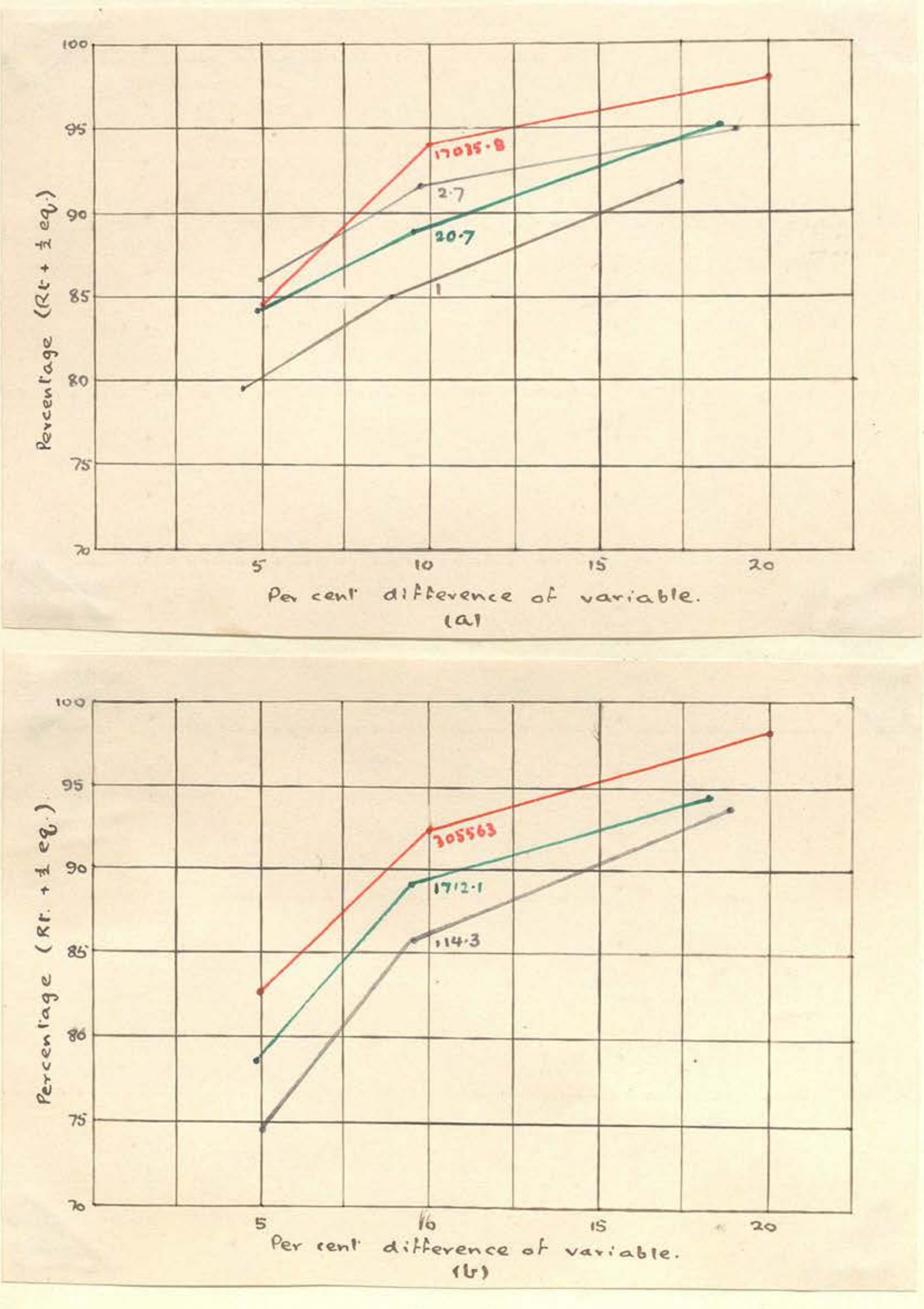


Fig. 9.

would give a meaningless result. This seems rather to conflict with the author's own conclusion ~~is~~ that in the whole of the sensation-scale investigated the percentage of Right judgements with a given stimulus-difference is essentially the same, so that differential sensibility remains constant from the weakest to the strongest sounds. Special attention is called to the fact that the statement holds good for the weak intensities. The conclusion seems to rest chiefly on the constancy of h - the measure of precision. This is questioned by Lorenz (27), who holds that this is not an adequate criterion. Lorenz also shows that sensibility may well have altered during the two semesters over which the investigation was made.

Nörr's experiment was much discussed by subsequent writers. The most important point raised was that of Tischer (44) who showed that Nörr extended Vierordt's (263) formula to cover heavier weights than those on which it was based.

At this point I propose to depart slightly from a strict chronological order, and deal with a group of researches which appeared from time to time in Wundt's Philosophische Studien between 1883 and 1900. Many of these are concerned only indirectly with the testing of Weber's law; a more general interest common to most of them is the measurement or discrimination of Schallstärken. Various types of apparatus, all based on the energy of falling bodies, were used. A direct comparison of results within this group is complicated by the fact that the different investigators used different formulations of the relation between intensity of sound/

sound on the one hand, and the height of fall and weight of falling body on the other. Most of the investigators, however, were attached to the Wundt laboratory in Leipzig, and were thus able to check each other's apparatus and results at first hand.

Tischer (44), in the first research of the series, calls attention to the inaccuracy of the formula $i = cph$. (i = intensity, p = weight of body, h = height of fall; c = a constant; these symbols are in common use in the articles under discussion.). A nearer approximation would be Vierordt's (263) proposed formula $i = cp\sqrt{h}$. Oberbeck (195), again, proposed $e = ph^{\varepsilon}$, giving a mean value of ε for the cases with which he himself had dealt. Tischer, however, found that it was impossible to obtain a formula $i = cph^{\varepsilon}$ with a constant value of ε, which, he shows, varies with height and weight as well as with the material of the base on which the bodies fall.

Tischer' work is in three parts: (i) experiments on the measurement of sound-intensity; (ii) a test of Weber's law by a form of the method of limits; (iii) experiments on time-relations in the judgement of intensity-differences. I shall deal only with part (ii).

The instrument used was Hipp's fall-apparatus, - the falling bodies lead balls of weight .3 - 100 gm., the height of fall 8 - 60 cm. The stimulus range (i.e. the ratio of the greatest and least intensities) was 800. The observer sat with his back to the apparatus, at a distance of from one to two metres. Stimulus values corresponding to an upper and a lower threshold were found by/

by the method of limits, starting each series at the equality point. When the weights differed, the required height for the variable stimulus was found from a table based on the results of part (i) of the investigation. The mean of the upper and lower thresholds was taken to give $\frac{\Delta r}{r}$.

Working first with one subject (Merkel) results were obtained giving values of the difference threshold between .36 and .44, with a mean of .4055. (see Fig. 10). Close agreement was hardly

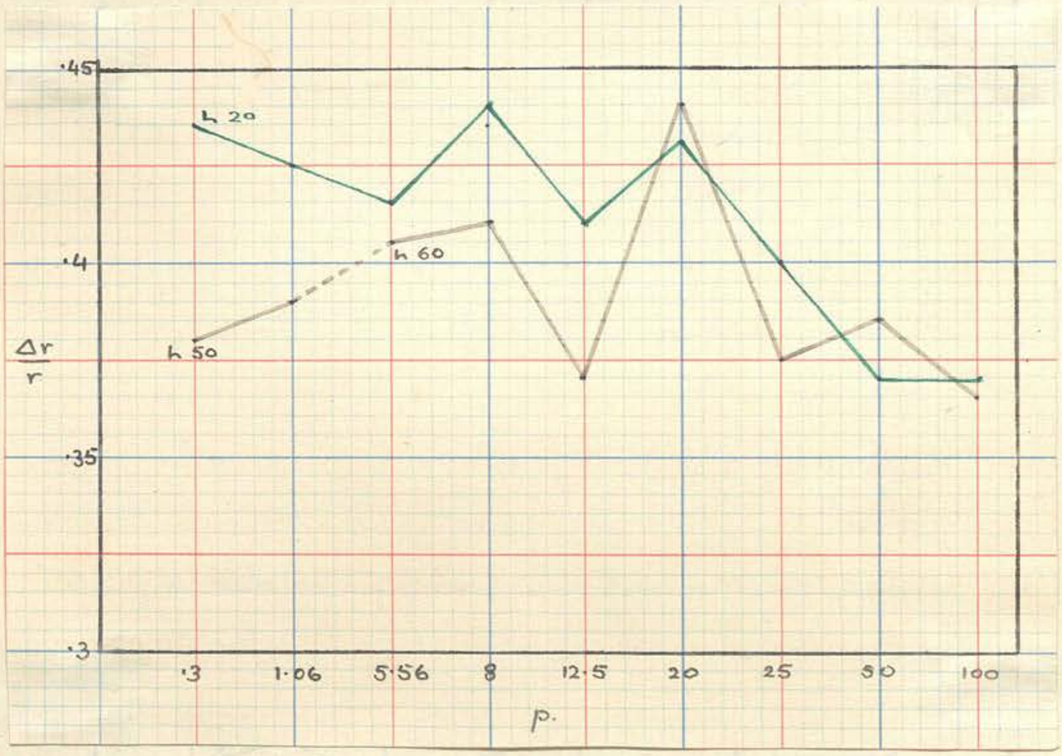


Fig. 10.

probable, since all the experiments were done within one period of three hours. The mean variation, however, was only about 5% of the mean.

A further series of experiments with four other subjects gave values which Tischer describes as '<u>so gut wie constant</u>'.

Two/

Two of the subjects, however, gave very high thresholds, and it was found that on retesting a general lowering of the threshold appeared with practice for all five subjects. The author supposes that it might have been lowered still more, had time permitted further experiments. The mean values available range between .363 and .452. No accurate conclusions as to variations with intensity can be drawn.

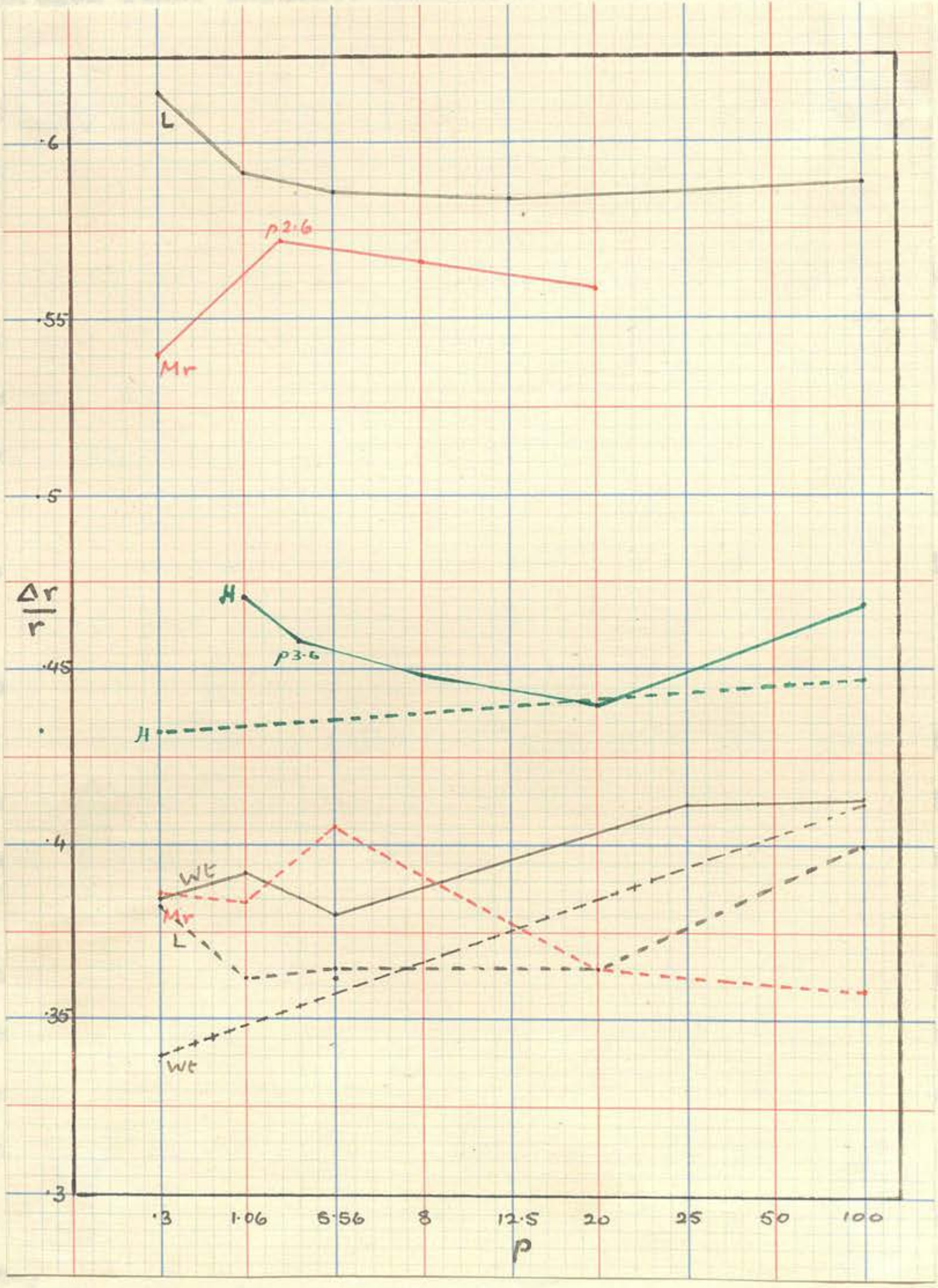


Fig. 11.

Fig 11 shows the principal data in graphical form. No attempt has been made to space the stimuli accurately according to intensity. Instead, I have plotted the principal standard weights irrespective of interval. The variable weight was always the same or nearly so. The broken lines indicate the retest. In the retest h was 20 for p = .3, 60 for p = 100, 40 for all other values. Variations are noted in the Figure; each of the four experimenters is represented by lines of an individual colour.

Lorenz (27), who was one of Tischer's subjects, was interested chiefly in the method of Right and Wrong cases, as applied to sound sensation; in particular, he compares Fechner's and Müller's calculation procedures.

Lorenz summarizes the previous work done by this method and severely criticizes that of Nörr (34), as I have already indicated, and that of Renz and Wolf (38), which, he says, was based on an erroneous calculation of intensity.

The apparatus was that used by Tischer. Two pairs of balls were used, each pair being in the ratio 2 : 1 (50 and 25 gm.; 25 and 12.5 gm.) and two heights, 20 and 30 cm. respectively. The intensities produced were in the middle range - i.e., no special attention was required in listening to them. Time-errors were eliminated by dropping the larger ball sometimes first and sometimes second. Alternation of direction of series was also observed. (It would appear that a variety of the method of limits was used, although the results were treated by the Right and Wrong Cases/

Cases process.). Conditions seemed to have a considerable effect; in particular it was found that the first sound seemed to lose in intensity. It appeared that judgements were influenced by a definite 'picture' which tended to become established - e.g. of a weak sound followed by a strong one.

Values of the difference threshold are contained in a short section at the end of the article, the upper and lower thresholds being given separately (see Table V).

TABLE V.

Weights	Height.	Upper Threshold.	Lower Threshold.
25 / 12.5	20	.32	.31
	30	.29	.29
50 / 25	20	.37	.32
	30	.32	.30

They are shown graphically in Fig. 12, along with results obtained by the method of limits, using eight pairs of weights. In drawing the curve I have averaged the upper and lower thresholds.

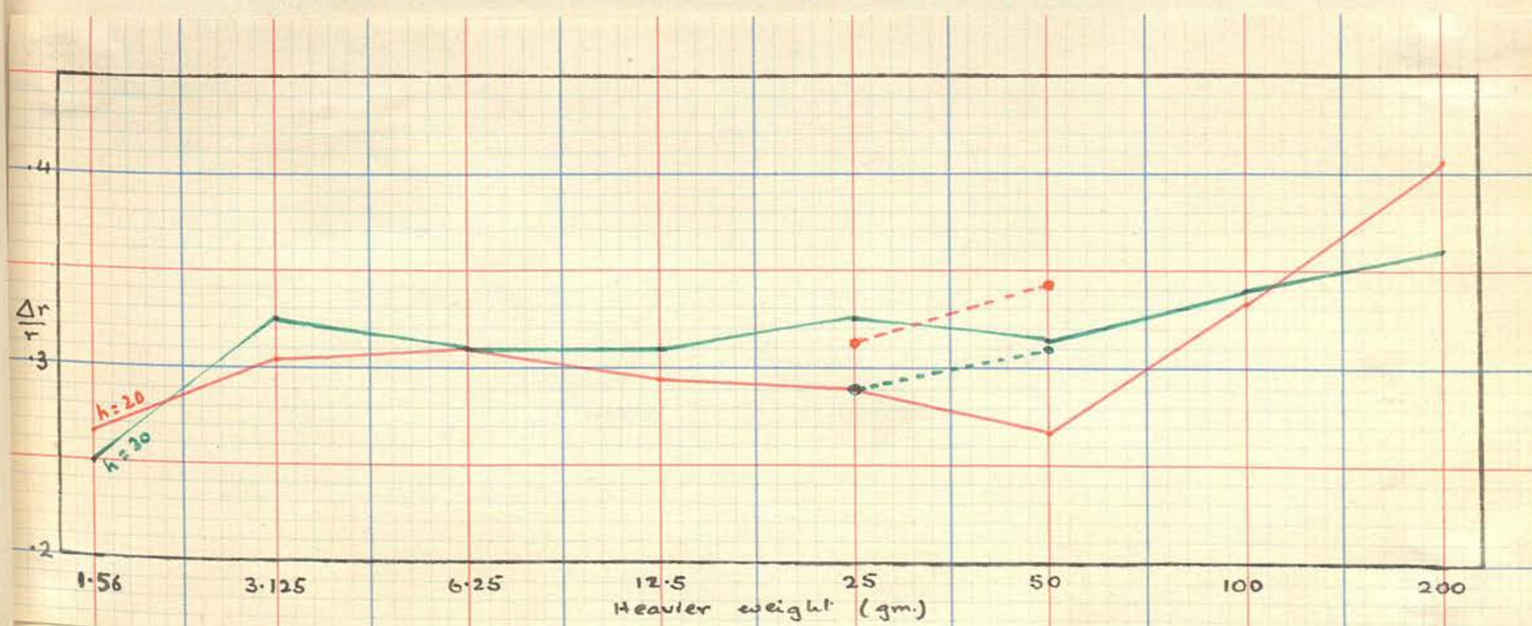


Fig. 12

The broken lines represent the data from Right and Wrong Cases; the continuous lines the Limits data. It will be noticed that there seems to be a slight tendency for the threshold to increase with intensity, but that the curves for the two heights used overlap to a considerable extent.

Most of the weaknesses of Tischer's investigation apply also to Lorenz. The same use of the intensity formula, depending on a separate value of ε for each case, was made. Most of the work was done with only one subject (Merkel) who had already acted as subject for Tischer. Lorenz, however, was aware that his results were insufficient for generalization. He characterizes the constancy obtained as fairly satisfactory, and states that it might have been better with greater care.

Starke (40) set out to explain more fully the inconsistencies in the theory of the sound-intensity of falling bodies discovered by Tischer. His paper includes a discussion of apparatus used in previous experiments, and introduces Wundt's improved Fall-phonometer (see Fig 3). One particular refinement stressed is the use of the Fangkaste for the reception of the balls after they had rebounded from the base-board. He also makes much of the difficulties of subjective sound measurement, in particular differences of quality (Klangfarbe) arising when sounds are "produced by different means", i.e., by using varying heights and weights. Starke himself always used a standard sound produced/

produced by a ball of 10 gm. weight, and variables produced by one, two, or three balls of 10 gm., or one of 20 gm. Originally he used lead balls falling on an oak board, but since these gave too much 'deformation', he substituted steel balls and an ebony board. The method was that of Limits, with complete ascent and descent.

Starke summarizes his results as follows:

(1) Sound-intensity is proportional to its kinetic energy (The testing of this hypothesis was his main object).

(2) A test of Weber's law based on this hypothesis confirms the law within the wide limits of the investigation.

The results in respect of Weber's law are not given in terms of a difference-threshold , but in terms of an error of estimation, obtained from the formula $\frac{h}{\eta} - 1$, where η is the point of subjective equality corresponding to a standard height of fall h . The values of this quantity for Starke's two subjects, one of whom was Lorenz, are given in Fig. 13. As will be seen, the

Fig. 13

stimulus-range is not so wide, corresponding to a ratio of rather less than 10.

Three years later Starke published another article (41) in which he checked his previous results, and came to the same conclusion. Intensity and kinetic energy were shown to be proportional to the height of fall when the weight was constant, and vice versa. In this second series of experiments the falling balls were made of ivory, and carefully matched in weight and volume, with the result that differences of Klangfarbe were negligible. Deviations from strict proportionality found by previous experimenters are explained as due to neglect of the effects of time-order and of the Weber's law estimation-error, i.e. the discrepancy between real and subjective equality.

Starke's results, on the whole, are somewhat imperfect, since, as Foucault (120) points out, he does not indicate the region in which the difference threshold reaches its minimum, nor does he show its increase with strong intensities.

The controversy on sound-intensity is continued by Merkel (31), who proposed a new formula, $i = p^{\eta} h^{\varepsilon}$. The presence of the exponents η and ε in this formula was explained by the fact that part of the energy is lost in deformation, rebound, etc. Different values of ε were necessary for each individual height used, not merely for different pairs of comparison stimuli. Merkel criticized the work of Tischer (44) and of Lorenz (27) on the grounds of total or partial neglect of this fact. A recalculation of Lorenz's results using this principle raised Lorenz's average/

average value of the difference threshold from .31 to .37.

Merkel makes a number of innovations in the general theory and method used.

1. In addition to the usual Weber's law formulation of the psycho-physical problem, it is possible also to ask by how much one must reduce a just noticeable difference to make it just disappear. Previous experimenters had neglected this aspect of the problem. Using an application of the method of limits, Merkel found that two different sound-intensities could be equated when their difference was .286 by diminishing the stronger, and when their difference was .200 by increasing the weaker.

2. A truer value of the difference threshold can be obtained by taking the geometric rather than the arithmetic mean of the upper and lower thresholds.

3. In determining sound-intensity, the error produced by the validity of Weber's law must be eliminated by multiplying by a Reduction factor R, obtained from the formula $R = \frac{2h - 2h_u}{ho}$ where hu and ho are the heights corresponding to the lower (<u>untere</u>) and upper (<u>obere</u>) thresholds respectively. (This of course assumes that equal weights are used, this being recommended by Merkel).

4. The method of Right and Wrong cases, being an 'error' method is quite different in its theoretical basis from that of limits. The former yields more reliable results, and a variety of it ('Equal and Unequal cases') can be applied to the investigation of conditions of apparent equality.

5./

5. The use of the Fallzange (i.e. 'pincers' - a device for ensuring constance of velocity etc. in dropping the balls) lowers the values of the difference threshold considerably. (see below).

6. The reciprocal of the difference threshold, i.e. $\frac{i}{\Delta i}$ gives a more useful measure of sensibility.

Merkel lists a number of other conclusions (more than twenty in all) but the above are the most important in their bearing on Weber's law. As regards the experiments themselves: Merkel used the same apparatus as Starke (with the addition of the Fallzange), and ivory balls of equal weight. Four groups of experiments were carried out, giving a total stimulus range of 10656. The results throughout are given as 'increase necessary' and 'decrease necessary'. Figures are also given for both time-orders, showing a very considerable effect of non-elimination of time error. In the table below I have also given general results for the difference threshold, taking both the geometric and the arithmetic mean.

TABLE VI.

	Upper Threshold	Lower threshold	Means.	
			Arithmetic	Geometric.
Group I.	.366	.267	.316	.312
II.	.363	.262	.312	.308
IIIa.	.318	.240	.279	.276
IIIb.	.308	.237	.272	.270
Average (as quoted by Merkel.	.360	.265	.312	.309
Using Fallzange	.310	.237	.273	.271

Two other minor conclusions are worthy of note. First, that a stimulus can be perceived with greater precision when presented along with one judged to be slightly (rather than distinctly) different, and preferably with a weaker stimulus. Second, that the influence of time-order varies inversely with length of time-interval.

The net result is a 'complete confirmation of Weber's law over a wide stimulus-range'.

A contrasting result was obtained by Merkel (32) in the third of a series of articles on the 'relationship between stimulus and sensation'. In a preliminary section he questions the validity of Starke's (40) proportionality result, referring in particular to the work of Stefanini (237), who found that the intensity of sound was proportional to the square root of the kinetic energy. Merkel shows that the value of ε depends on the ratio of the weights used, but is unable to draw conclusions with respect to the relation between intensity and height.

The main portion of the article is concerned with an investigation of sound-intensity by the method of Mean Gradation (<u>der mittleren Abstufungen</u>). In this three stimuli are presented, of which the first and third (the 'terminal' stimuli) are in some fixed ratio. The dhief ratios used by Merkel were 3, 5, 6, 10, and 15. The aim of the method is to find a 'mid-point' between the terminal stimuli. This is usually represented by the symbol R_m. The terminal stimuli are called R_1 and R_2, and R_a and R_g represent resepectively the arithmetic and geometric means. If Weber's law holds the estimated mean should coincide with/

with, or at least approximate to the geometric mean. **Fg** (for Fehler) is used to measure the deviation of the estimated from the geometric mean, and is given by the formula $F_g = \frac{R_m}{R_g} - 1$ F_a ($= \frac{R_m}{R_a} - 1$) , similarly, represents the deviation of the estimated from the arithmetic mean.

Merkel's experiments were carried out with balls ranging from .45 to 164 gm., the apparatus etc. being the same as in his previous experiments. Some typical results are given in abbreviated form in Table VII, below.

TABLE VII.

R_1	$\frac{R_2}{R_1} = 3$		$\frac{R_2}{R_1} = 5$		$\frac{R_2}{R_1} = 10$	
	F_g	F_a	F_g	F_a	F_g	F_a
2.025	.157	.002	.358	.012	.779	-.023
4.993	.146	-.006	.338	-.003	.662	.016
9.886	.161	.006	.319	-.010	.788	.028
39.73	.169	.012	.329	-.009	.678	-.035
77.89	.149	-.005	.330	-.009	.672	-.039
146.6	.203	.042	.330	-.009	.629	-.061
260.8	.161	.006	.326	-.012	.613	-.073
795.2	.162	.006	.310	-.083		
1234	.152	-.003	.295	-.094		

From these figures it will be seen that F_g is invariably greater than F_a , i.e., that the estimated mean approximates much more closely to the arithmetic than to the geometric mean. It/

It further appears that F_g tends to increase with the ratio $R_2 : R_1$. This is also true of F_a , though not nearly to the same extent. F_a also seems to tend to become negative with increase in ratio between the terminal stimuli, i.e. for a smaller ratio the estimated mean tends to be higher than the arithmetic mean, while the opposite holds for an increased ratio. The absolute value of the stimuli seems to have no bearing on the results.

It would appear, therefore, that the method of mean gradation, at least, gives results contrary to the requirements of Weber's law. Merkel concludes by drawing up a generalized law for the relation between stimulus and sensation, which, he claims, holds for Light, Weight, and Sound. Sensation (<u>Empfindung</u>) is given by the equation

$$E = c. \left[\frac{b + mR}{n + R} \cdot R \right]^{\frac{1}{\varepsilon}}$$

Where c is an apparently indeterminable constant; b, m, and n empirical constants, different ~~from~~ for the three sense-departments named; and $\frac{1}{\varepsilon}$ has the value 1 for Sound, and $\frac{1}{2}$ for Light. Merkel claims that in 86% of cases the difference between observed and calculated values is not more than .01 R , though it is not quite clear how this conclusion is reached, in view of the 'indeterminability' of c .

Along with the work of Merkel just described it is necessary to deal with that of Angell (3). I deal elsewhere with Merkel's claim for the validity of the <u>Verhältnisshypothese</u>, but it may be/

be as well here to note that while the results obtained by Merkel seem to support it, those of Angell point to the opposite conclusion, namely, the validity of the Unterschiedshypothese which is equivalent to a confirmation of Weber's law. The Merkel-Angell controversy seems to have aroused considerable interest at the time. In view of their conflicting experimental results it is a little difficult to come to a definite conclusion. The whole topic is discussed at length by Ament (2, q.v.) Meanwhile it must suffice to give a brief resumé of the results, and to add that the controversy is now all but forgotten.

Like Merkel, Angell made a number of advances on current theory and practice. His statement of the status of Weber's law is given elsewhere. His other contributions are chiefly in the nature of experimental refinements, including a month's preliminary practice in judging sound intensities, and a careful investigation of the precautions necessary to obtain sounds identical in quality. His apparatus and methods were similar to Merkel's; four ivory balls of as equal weight as possible were used. His subjects, three in number, found great difficulty in making their judgements, especially in the 'Method of Doubled Stimulus', which Angell later discarded as unsuitable to the problem in hand. In the method of mean gradation Angell found a tendency for the judgement to have formed before the presentation of the third stimulus. He also found it impossible to calculate the exact proportion of energy transformed into sound-vibrations. Later Angell enlarged his group, and the already strict experimental conditions were made still/

still stricter.

Most of his conclusions, however, are made on the basis of the results of two observers. Their results, for five pairs of terminal stimuli, are given in Table VIII.

TABLE VIII.

$R_1 : R_2$	G.M.	A.M.	Estimated Mean Ke	Estimated Mean Kr
10 : 40	20	25	19.62	20.49
20 : 60	34.6	40	35.00	35.75
15 : 60	30	37.5	28.60	32.33
20 : 80	40	50	41.61	43.71
20 : 100	44.7	60	43.77	51.11

Angell also gives the results for one observer (Ke) in a curious graphical form. Two of his five figures are reproduced,(Figs 14, 15)

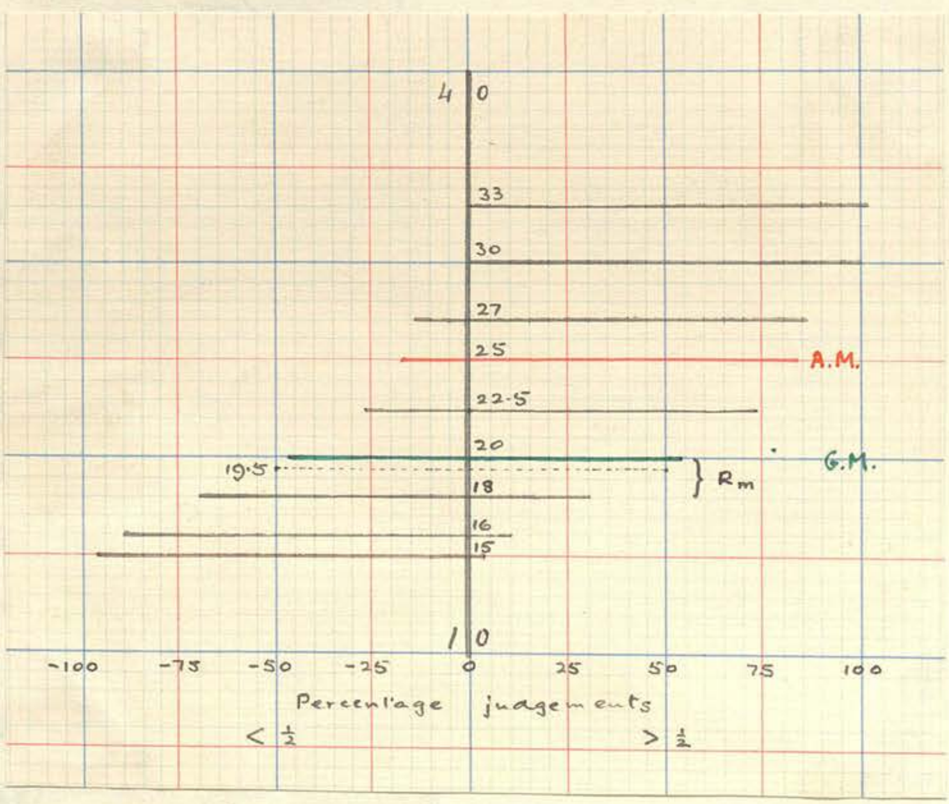


Fig. 14

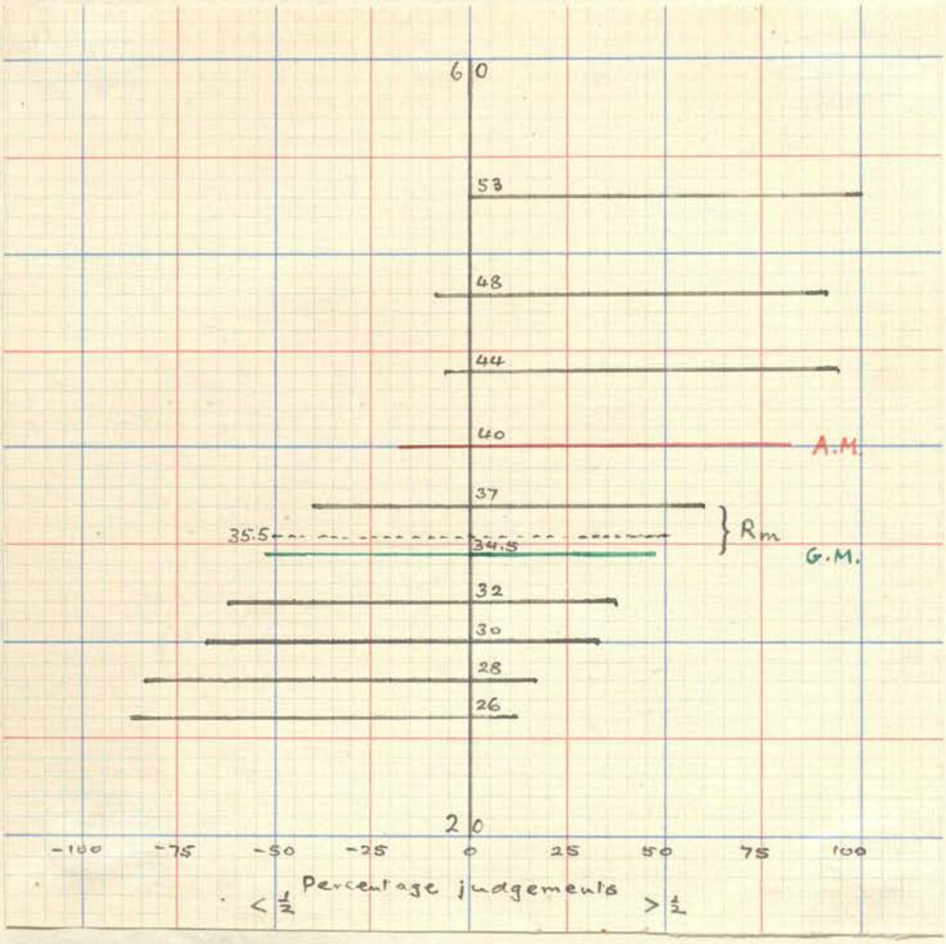


Fig. 15.

The large figures (10, 40 and 20, 60) represent R_1 and R_2. The other ordinates show the various 'mid-stimuli' which had to be judged as less (negative direction) or greater (positive direction) than the subjective mean of the terminal stimuli. The abscissae at these points represent the percentage of 'less' and 'greater' judgements. The arithmetic and geometric means are shown, in my diagrams, by coloured lines. The brace indicates the range within which the estimated mean falls, and the fine dotted line its actual position, obtained by Wundt's interpolation formula/

formula. (Judgements of 'middle' were divided between the categories 'greater' and 'less', and the value giving 50% of each was taken as the estimated mean). This method of representing the results has the advantage that it enables one to see at a glance the relations between R_m, R_a and R_g.

The author shows that R_m is invariably nearer the geometric than the arithmetic mean, but that the approximation shows no regular variation with the ratio of the terminal stimuli, nor with their absolute values. The general finding thus amounts to a good correspondence with the requirements of Weber's law. In addition, Angell shows the great effects of accommodation and expectation when the stimuli are presented in some regular order, as in the method of limits.

A paper by Kämpfe (19) is concerned chiefly with an experimental test of the method of Right and Wrong cases. It also contains, however, a section on the measurement of sound-intensity by the use of falling bodies. Kämpfe gives a symbolic relation

$$L - R - (W + A + D) = J$$

where L = total energy

R = energy of rebound

W = air resistance

A = friction at point of release

D = deformation

and J = intensity.

For small heights $(W + A + D)$ is practically zero, and other components may be neglected, so that in general it may be said that/

that $J = L - R$. It is thus shown that the complete proportionality between height and intensity assumed by earlier experimenters, including Fechner (7), (who gives a table based upon it), is erroneous ~~enormous~~.

Kämpfe reverted to the use of the sound-pendulum. He experimented first with that of Volkmann. In this the essential parts were made of steel. It was found, however, that this instrument produced 'persistent echoing and buzzing' noises, which made judgement impossible when the observer sat near. Accordingly a new model was substituted, with vulcanite balls mounted on wooden arms, and striking against a block of oak, which was glued instead of screwed to the base. The arms were damped with felt, and later one arm was discarded, so that the pendulum, originally double-armed, became virtually single-armed. A device was introduced which made it possible to use two release points. A fine steel thread fastened on the ball with wax gave a direct measure of rebound.

Although Kämpfe endorsed Angell's opinion that an exact determination of the fraction of energy converted into sound is impossible, he found a correspondence with the formula $\sin^2 \frac{A}{2} = C \sin^2 \frac{\alpha}{2} + C'$ (where A and α are angles of fall, and C and C' constants) established 'to a degree hardly expected'. A table of intensity differences corresponding to increments of $\frac{1}{2}^{\circ}$, 1° etc. over standard heights of 30°, 40°, 50° and 60° is given.

Kämpfe experimented with both himself (<u>wissentliche Versuchsverfahren</u>), and with others (<u>unwissentliche</u>) as subjects. Results/

Results having a direct bearing on Weber's law are given for all cases, but not in terms of a difference threshold. Instead, the constancy of the product hi (h = measure of precision; i = intensity) is taken as criterion. This constancy is shown to hold good, within the limits of a Probable Error of 3 - 6%. Weber's law is accordingly taken to be valid over a wide range.

The work of Mosch (33) is of no great importance. I include it here for the sake of completeness, and because it suggested a new method, later developed by Keller (20). At the time of its publication interest in the psychophysical measurement methods seems to have flagged. Mosch himself notes that nothing new had been done for five years, i.e., since the work of Kämpfe, just quoted. A number of procedures were available, however, for the handling of Right and Wrong cases data, and the choice of formula was apparently a matter of personal taste. All of these rested on the Gaussian law of error, and Mosch's aim was to test its applicability in psychophysics.

used ... of judgement:
Mosch [applied to sound-stimuli the] five categories [used by Wreschner (286) in his work on lifted weights. The categories were] 'much greater', 'greater', 'equal', 'less', and 'much less'. Mosch holds that there is no question as to the more exact treatment possible by this method. Otherwise the method was that of Right and Wrong Cases.

The apparatus was Wundt's fall-phonometer, with ivory balls. All four available sections of the phonometer were used, and a dozen/

dozen ebony boards made, as identical as possible. Of these four were selected, but even so slight differences of timbre were detected by two of the four observers. Other precautions to eliminate extraneous differences were taken, including rotation of the four *Apparate*, so that each served its turn as standard. The balls were released by opening the *Klemmvorrichtung* ('pincer-device') by hand, so as to save time. Sufficient preliminary practice was given, and the other experimental conditions were made as rigorous as was thought necessary. It was found that all the subjects gave the same time-error, viz., an over-estimation of the second stimulus. This error was eliminated in drawing up the results.

Since the conclusions with respect to Weber's law are purely negative, I have not thought it necessary to go into the mathematics of the method, based on the work of Bruns (~~470~~ 90 and elsewhere). Suffice it to say that Mosch found that the magnitude U (measure of uncertainty) was not suitable for a test of Weber's law. He expressed the hope, however, that the extension of his data might make it possible to discuss Weber's law on the basis of certain other values. He further hoped that his work might give a new impulse to discussion of psychophysical methods, especially as his results indicated that the Gaussian law in its simple form is not sufficiently accurate to represent his observed data. The new categories of judgement were shown to be, on the whole, reliable, and definitely different from the simple 'greater' and 'less'.

The/

The last article of the series under discussion came seven years after Kämpfe's, at the turn of the century. Ament's work (2) on just noticeable differences contains a review of much of the previous work, and also gives original results which in a sense foreshadow future developments. It may therefore conveniently be taken as a limiting point, although the series may be said to continue in the <u>Psychologische Studien</u>. A discussion of these later studies has been postponed to their proper chronological position.

Ament's main problem is the relation between the just noticeable and the supraliminal (<u>übermerkliche</u>) difference. Two procedures for determining this relation are described. The first, the 'direct' method, is to construct a stimulus-scale in terms of just noticeable differences, and to use the values thus obtained in the comparison of supraliminal differences; the second, or 'indirect' method consists of a comparison of ratios etc. obtained by the normal psychophysical methods, including mean gradation. The theory of these methods need not detain us, nor need their direct application. A number of results more closely connected with Weber's law, however, are also given.

Results obtained by Merkel (31) are recalculated and given in the form $\frac{\Delta r}{r}$, expressed as a fraction of the form $\frac{1}{n}$. Incidentally, Ament appears to have made a few errors in his calculations. In drawing the accompanying curve (Fig. 16) I have corrected these, and converted the fractional values to the more/

more customary decimal fractions, extending the range given by Ament to the whole of the relevant portion of Merkel's results.

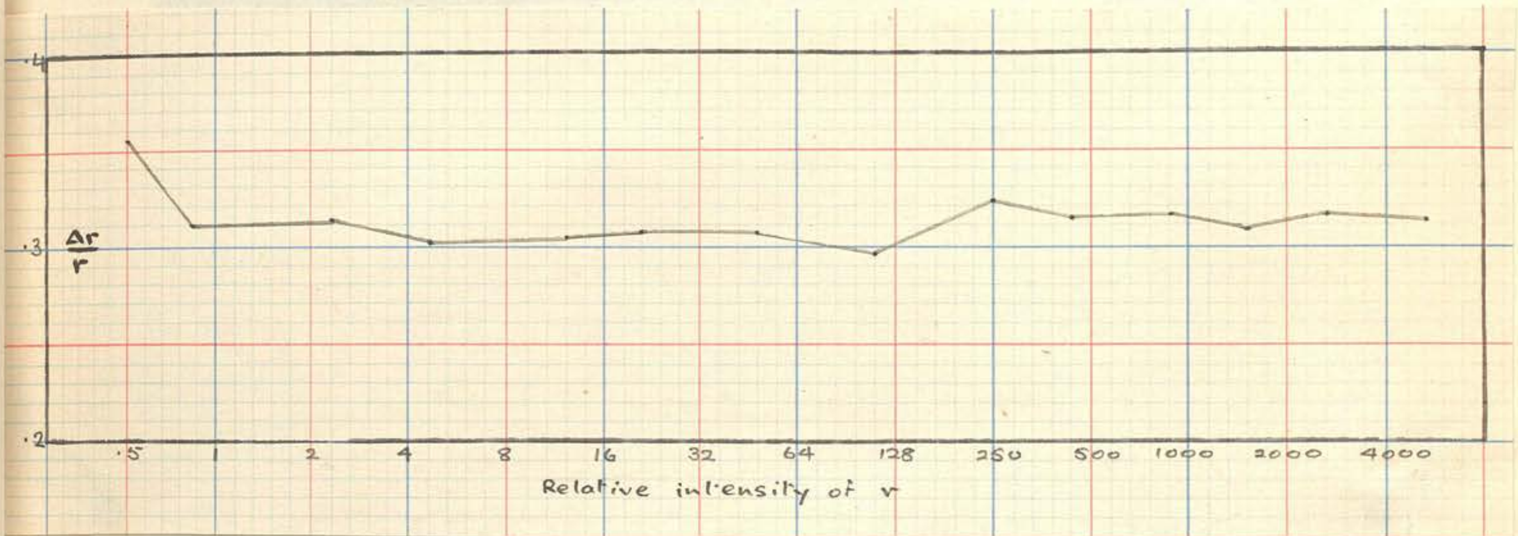


Fig. 16

(The abscissae of the curve are roughly plotted on an approximately geometric scale). It will be seen that the values of $\frac{\Delta r}{r}$ fluctuate between about .295 and .355. Except for a very slight tendency to higher values for the lowest intensities, the variation seems to be irregular.

Ament deals at some length with the Merkel-Angell controversy. He shows that the values obtained for R_m are largely a function of those of R_1 and R_2 - of both their ratio and of their absolute values. When R_1 and R_2 are close together, in either sense, there can be little doubt as to the mid-point. When, on the other hand, the differences are greater, each stimulus tends to be judged more and more in relation solely to the immediately preceding stimulus. The bearing of the equality or otherwise of j.n.d.'s on this question is fairly evident.

We/

We come now to Ament's own sound-experiments. The apparatus used was that of Kämpfe (19), with slight modifications. The time-interval ($1\frac{1}{2}$ seconds) was chosen in accordance with what the subjects judged to be most suitable. The methods were those of limits and mean gradation. Following Külpe (163), (from whose Würzbung laboratory this research was issued), Ament decreased the size of step in the region in which the turning-point of judgements was expected. Trials were repeated when necessary. The subjects were kept in as complete ignorance of the situation as possible. Ament claims that control experiments showed that all his precautions were justified.

The results for two subjects by the method of limits are given in Fig 17. Ament states that they show apparent constancy for for a stimulus-range of about 3 - corresponding to the range $27^{\circ} - 47^{\circ}$. At 67°, he says, there were too many 'distracting effects'. The two subjects, however, gave widely different results: Subject A the usual value of about $\frac{1}{3}$, while subject K gave a mean value of $\frac{1}{6}$. Considerable stress is laid on this individual difference, and I think that this is significant, since previous investigators tended to interpret any apparent deviations rather in the light of experimental errors. Ament's results show the beginnings of an interpretation of the 'Weber-Fechner ratio' as a variable rather than a constant function. They also show a steep drop in/

in $\frac{\Delta r}{r}$ when the weakest intensities have been passed. This is typical of later work.

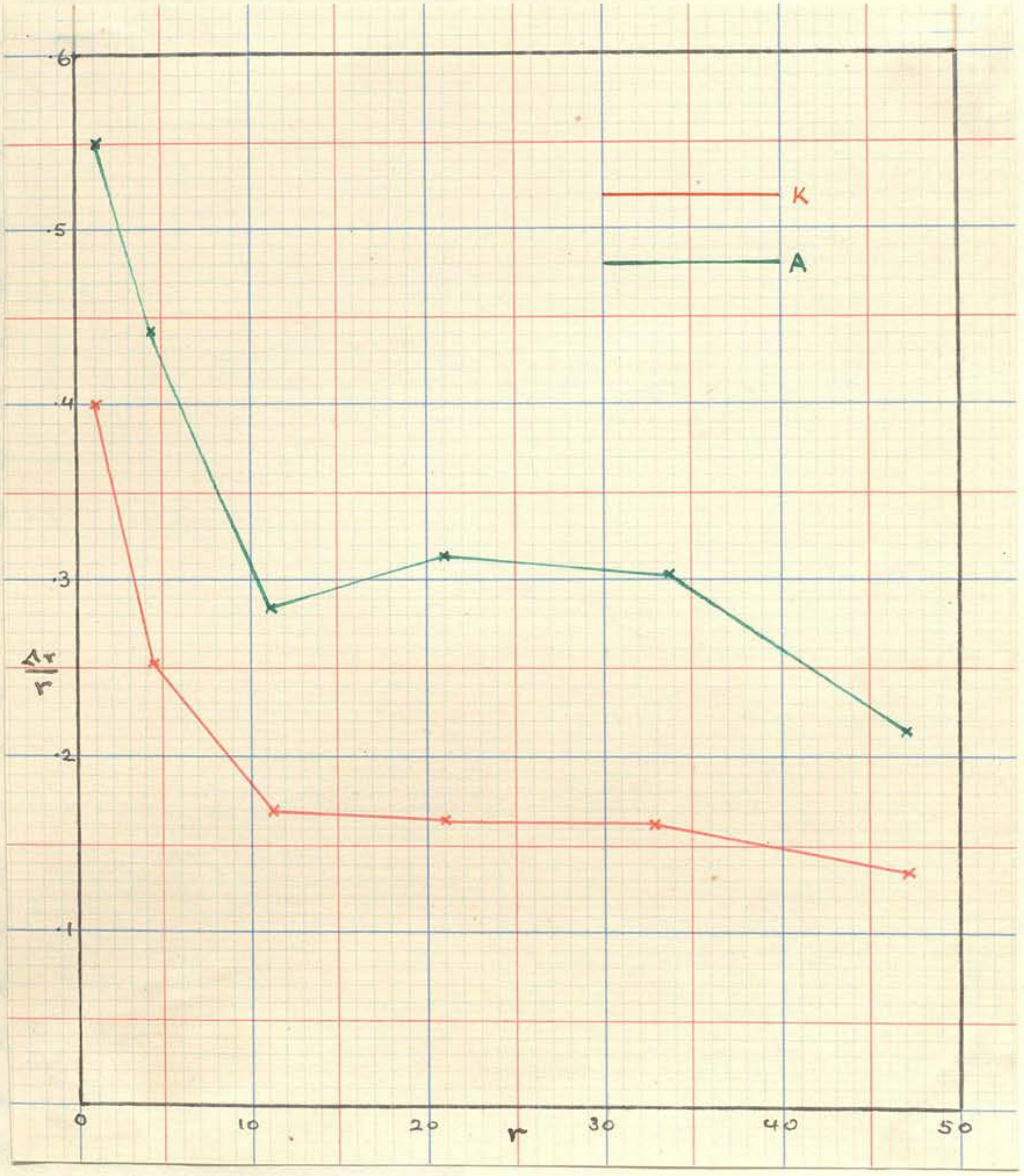


Fig. 17

Mention has already been made of Ament's conclusions on the method of mean gradation. His own results are summarized in Table IX.

TABLE IX.

$R_2 : R_1$	Regular variation				Irregular variation			
	K		A		K		A	
	Fg	Fa	Fg	Fa	Fg	Fa	Fg	Fa
46.95 : 1	1.16	.38	1.84	.19	1.09	.40	1.30	.34
32.78 : 1	.63	.45	1.08	.29	.84	.38	1.14	.28
20.76 : 1	.48	.38	.79	.25	.59	.33	.52	.36
11.24 : 1	.21	.32	.46	.20	.24	.32	.19	.35
46.95 : 11.24	.04	.18	.14	.10	.10	.13	.13	.11
46.95 : 4.50	.44	.19	.61	.09	.64	.07	.53	.14
32.78 : 4.50	.17	.23	.24	.19	.41	.08	.30	.09

Ament carried out two series of experiments: the first with regular variation of the middle-stimulus, comparable with the method of limits; the second with irregular variations, comparable with Right and Wrong cases. These are given side by side in the Table, and it will be seen that K's figures check more closely than do A's. Other individual differences are as follows: In an experiment with the 'direct' procedure (see above) K distinguished twice as many j.n.d.'s between the terminal stimuli 1 and 46.95 as did A. This tallies with K's fine discrimination in the method of limits. It is, however, noteworthy that whole the results of both subjects usually show a closerapproximation to the arithmetic than to the geometric mean, A shows only one case of Fa $>$ Fg, as against five/

five for K. In experiments designed to show the effect of contrast and time-order K showed more effect of the former, A of the latter. Finally, it may be said, in general, that Fg decreases regularly with decreasing ratio of $R_2 : R_1$, and F_a less regularly.

Taken all over, Ament's results are somewhat negative and inconclusive. On the other hand, they are fairly important for other reasons, already indicated.

By way of postscript to the experiments detailed in the Philosophische Studien, I quote Wundt's own conclusions, as given in his Grundzüge (46). Sound-intensities, he holds, afford suitable material for testing the 'sensation-law', particularly in respect of the short duration of the sensations, and the small degree of fatigue of the organ. On the other hand, the difficulties of measurement are well-known. Of the authors quoted, only Starke (40, 41) found a direct proportionality between intensity and height and weight. Such a proportionality can only be safely assumed when the deformation of ball and base-plate is small. Different methods give different results, and each method is limited in accuracy by its own peculiar type of error. The general result, however, is that the method of limits yields a wide validity of Weber's law, while the method of mean gradation gives a closer approximation to the arithmetic than to the geometric mean. Nevertheless, Wundt states that hearing is the sense department in which Weber's law is of the widest application.

At this point it is necessary to retrace our steps a little, and mention one investigation which I have passed over in dealing with the Philosophische Studien as a unit. A few other articles having some bearing on the subject, but rather indirectly, were published during this period. These are mentioned elsewhere.

The investigation in question is that of **Wien** (45), originally published as a dissertation for a doctorate. **Wien's** research is of special interest in that it deals with tones, in contrast to all those already discussed, which deal with noises. **Wien's** main purpose in adopting this course was to overcome the difficulty of loss of energy in the case of noises. Reference is made to the work of Oberbeck (195), but **Wien** shows that Oberbeck's results are not very applicable to tones.

Wien's apparatus consisted in essence of a resonator, covered with the membrane of an aneroid barometer, the whole approximating to an artificial ear-drum, and constituting an appliance with which the relative amplitude of vibrations could be measured with great accuracy. It was thus possible to measure intensity in absolute units. The source of sound was an electrically-driven tuning fork of a given frequency, and the tone was presented in a telephone receiver, at varying distances from the receiver. The apparatus was shown to be accurate within an error of not more than 5%.

Wien formulated three problems, of which we are directly concerned only with the first: "In what ratio does sensation increase with the intensity of the stimulus?" This of course amounts to a test of Weber's law. The method of limits was used; a check for 'subjective elements' was made by the method of Right and/

and Wrong cases. The check results are given in Fig. 18 (marked RW), and in Table X. Limits values corresponding to the intensity 2×10^3 were apparently obtained by interpolation, and are given in the table in square brackets.

TABLE X.

R	ΔR/R				
	Limits.			Right and Wrong Cases.	
	220	337	440	220	440
5			.135(?)		
20			.108		
10^2	.182		.112		
10^3			.118		
2×10^3	[.195]		[.116]	.213	
10^4			.116		.142
10^5	.224	.176	.131		
10^6			.140	.283	
10^7			.153		
10^8	.270		.161		
10^9			.178		
10^{10}			.225		
10^{11} (?)			.350		

Each Limits series was given 40 times.

Three tones were used - 220, 337, and 440 cycles per second, at 3, 1, and 13 intensities respectively. The intensities were in powers/

powers of 10, and herein Wien appears to have anticipated the decibel scale, except that his scale is such that the threshold of hearing is at 1.6. The results are given in table Table X and in Fig. 18, and seem to show a rise in the difference-threshold with

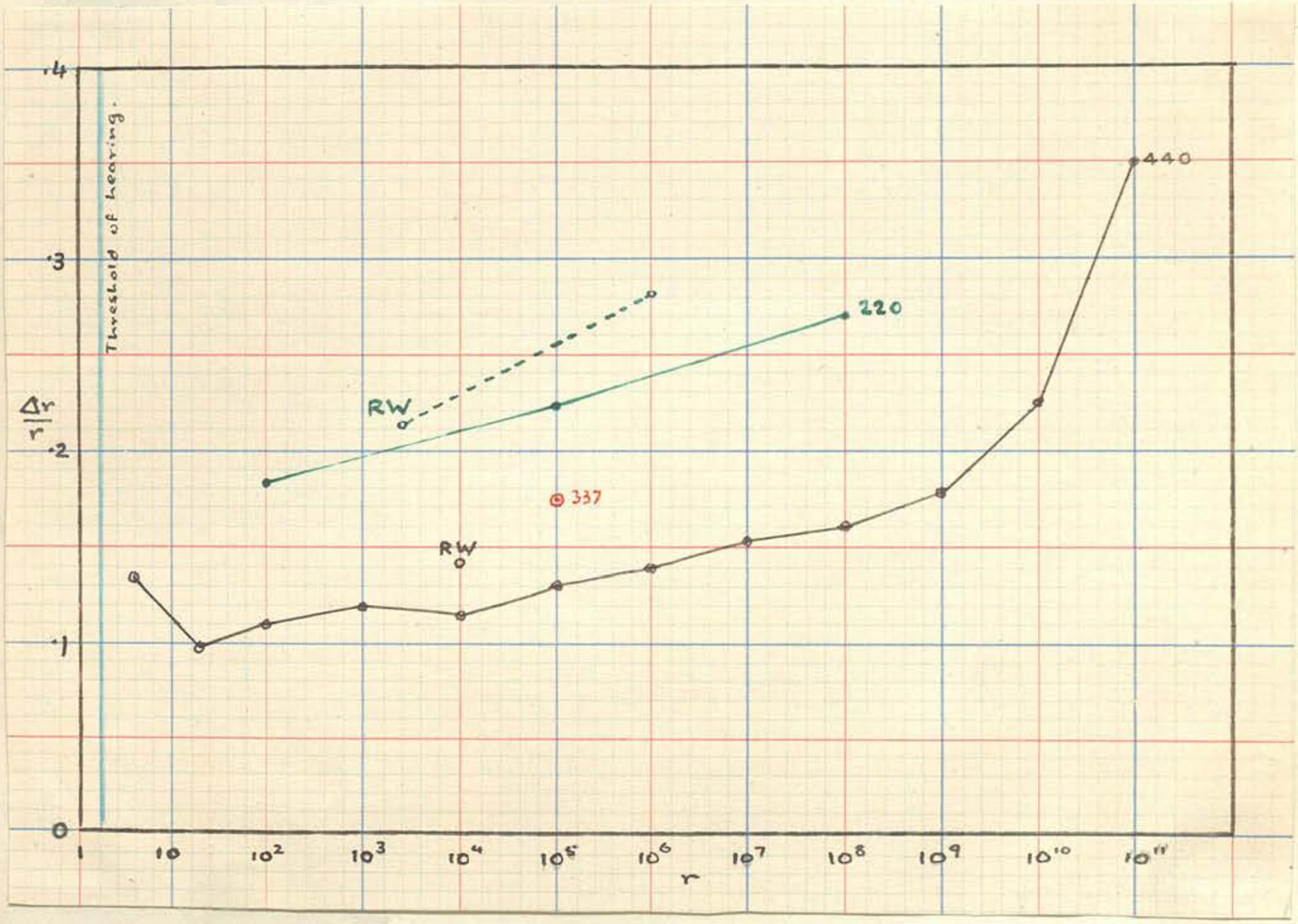


Fig. 18

increased intensity, except for a slight drop at very low intensities. Wien's own conclusion was that Weber's law was approximately valid: .. '<u>gilt überall annähernd</u>, <u>nirgends genau</u>'.

On the basis of these results Wien draws up an empirical equation:

$$\Delta E = c\left\{\frac{\Delta R}{R} - \frac{1}{40}\cdot\frac{\Delta R}{R}\log R - \frac{1}{400}\frac{\Delta R}{R}\log^2 R\right\}.$$

In its integrated form, this gives

$$E = c\left\{\log\frac{R}{a} - \frac{1}{80}\log^2\frac{R}{a} - \frac{1}{1200}\log^3\frac{R}{a}\right\}$$

Putting R/a = 10 and E = 1 , Wien obtains the table:

R/a	1	10	10^3	10^6	10^9	Reizhöhe (10^{12})
E	0	1	2.9	5.4	7.7	c. 9.0

This gives a curve for the relation between stimulus and sensation, comparable with the loudness curves discussed later. The curve is given in Fig 19; a slight irregularity appears at

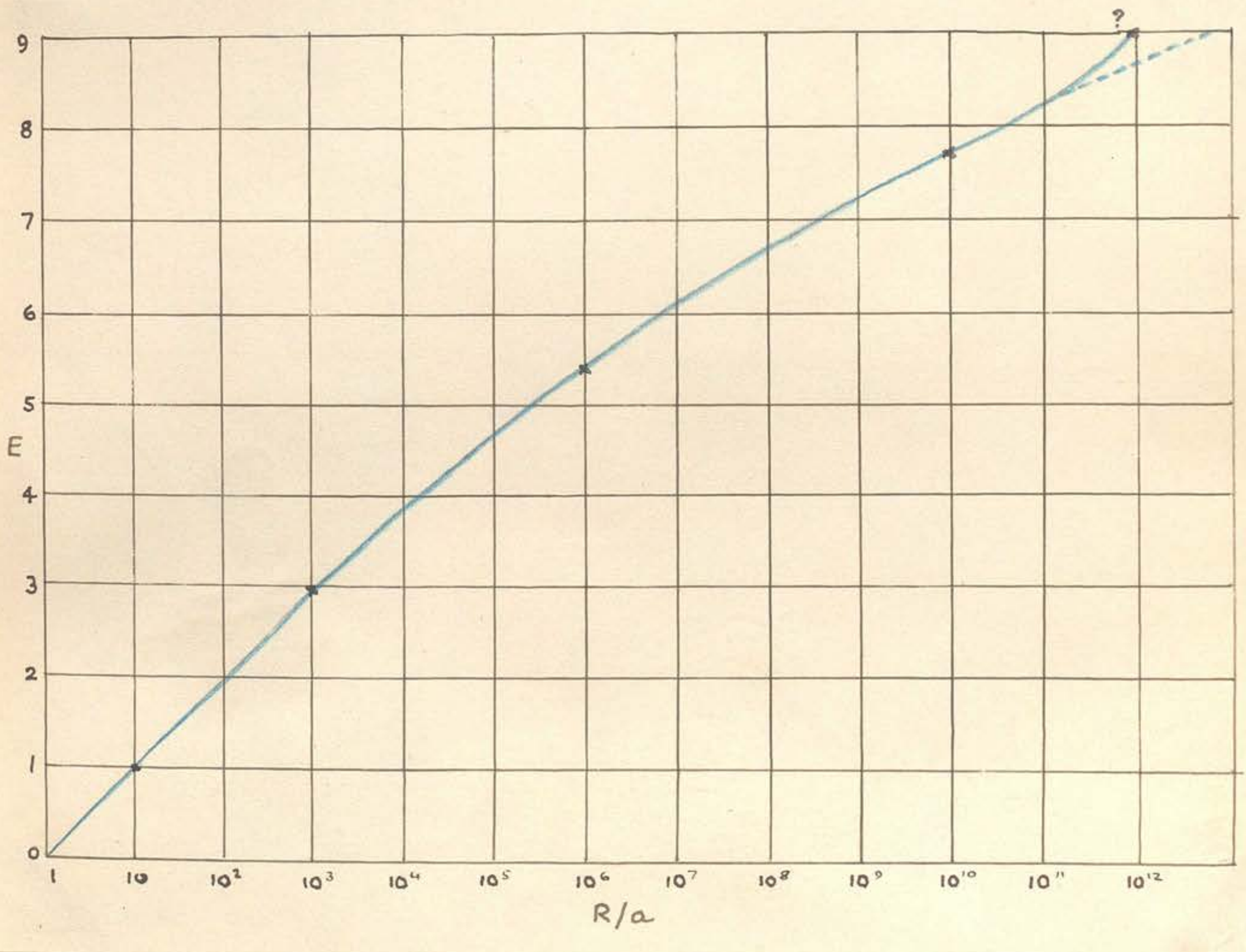


Fig. 19

the upper extreme, i.e., at the point at which Wien gives 10^{12} as a rough upper limit, with a sensation-value of about 9.

Variation of intensity-discrimination with frequency is dealt with briefly. Wien proposes that the sensibility of the ear for tones of different pitch be measured by the reciprocal of $\frac{\Delta R}{R}$. Taking a tone of 440 cycles as standard, and alloting it the value 100, Wien gives the following figures for comparative sensibility at a medium level (10^5 on his scale):

Tone	Sensibility.
220	58.5
337	74.4
440	100
Noise	37.4

The noise figure is based on the results of Vierordt (263) and others. Sensibility is found to grow fairly rapidly with pitch.

Wien's work is important in at least three respects:

(1) The use of tones, because (a) the intensity could be objectively determined, and (b) comparison with the results of other investigators could be made with a greater degree of certainty.

(2) The wide stimulus-range, covering intensities from near the absolute threshold to near the upper limit of hearing.

(3) The development of a sensation-scale, as described above.

In 1904 Hoefer (18) made another new departure, which does not appear to have been followed up since. Hoefer studied auditory differential sensibility among individuals suffering from psychoses and/

and functional neuroses.

The apparatus was the simple fall-phonometer used by Starke (40), but with a base-plate of zinc. This was chosen to reduce differences of timbre to a minimum, but these could not entirely be eliminated. The method of Right and Wrong cases was used with 'equal' and 'doubtful' cases divided among the 'rights' and 'wrongs'. Heights varying from 325 to 1300 mm. were used. It was found that the value representing the crucial test of Weber's law - $n\,G_n^X$ (n being the measure of precision and G_n^X the standard stimulus corresponding to a height n) was constant only for relatively large difference of stimulus.

Subnormal sensibility was found in only a few cases, though flagging of attention was often evident. The disorders studied comprised Epilepsy, Dementia hebeprenica, Dementia paralytica, Melancholia, Paranoia chronica, Neurasthenia, and Hysteria. Hoefer stresses the difficulties encountered, and holds that at least 3000 trials with each subject are necessary to give satisfactory results.

The work of Deenik (6), reported originally by Zwaardemaker, represents an advance on that of Wien in respect of the frequency range covered. Unfortunately, however, the intensities used are not so conveniently graded.

Deenik starts from a review of Wien's work, and gives the following Table of Wien's results:

TABLE XI./

TABLE XI.

Tone	Average Threshold	Extremes.
a (220)	.225	.182 - .270
e (337)	.176	
a (440)	.144	.108 - 225

His own experiments fall into two groups, using (a) tuning-forks, (b) organ-pipes.

The tuning-forks were electrically-driven, and maintained at a fixed amplitude, which was measured microscopically. The observer was seated in a 'sound-free' cabinet, and the sounds were led in by means of thick rubber hearing-tubes. Alterations in intensity were produced by rotating the tubes in Kiessling's interference-planes, the angles being later converted into absolute values. The results are given in simplified form in Table XII and

TABLE XII.

Tone	Ampl. in microns	$\Delta r/r$	Average.
c^1 256	640	.296	
	800	.344	.332
	1040	.357	
c^2 512	20	.227	
	40	.269	
	70	.298	
	100	.308	.293
	150	.310	
	200	.315	
	300	.320	
c^3 1024	2	.234	
	2	.202	.195
	2	.149	

Figure 10. The threshold in each case is a lower threshold, expressed as an average of five determinations by the method of limits.

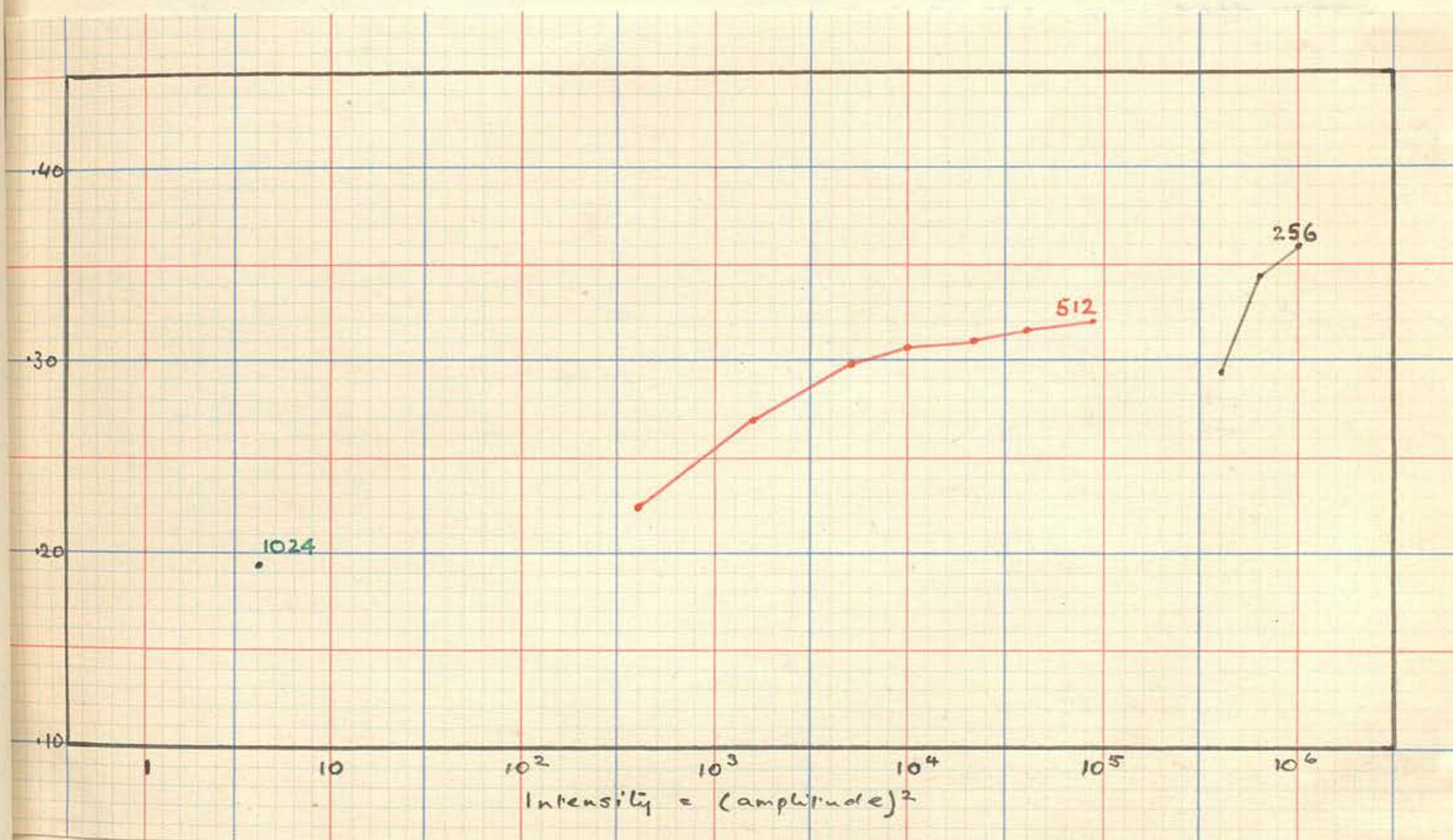


Fig. 20

The organ-pipe apparatus is described as follows: An accurately tuned wide covered wooden organ pipe was placed in a felt tent in a room adjacent to the 'sound-free' cabinet, as in the tuning-fork experiments. The pipes were continuously blown by a water-driven pressure-pump. The air was dried with calcium chloride, and supplied through a long system of lead pipes. The sound/

sound was conducted through two rubber hearing tubes with diaphragm openings adjusted by micrometer screws. The subject himself adjusted one diaphragm until the sounds appeared different. Five limits series were given as before, but in this case both upper and lower thresholds were found. Meters for pressure and volume of air were included in the set-up, and energy was calculated from the formula:

Energy = air vol. x pressure x 981.

This, multiplied by a constant factor, different for each pipe, would give acoustical energy in absolute units. This Deenik did not do, since absolute values were not necessary for the calculation of the difference-threshold for each tone, and Deenik was interested chiefly in the variation of differential sensibility with pitch. The results are given in Table XIII. The intensities are 'relative' apparently only for the tone to which they belong; in the table I have given these figures correct to a whole number.

TABLE XIII/

TABLE XIII.

Tone	Rel. intensity of standard.	Thresholds. Upper	Lower	Ave.
C 64	1392 1121	.237 .236	.219 .237	.232
G 96	1560 1243	.199 .211	.199 .210	.204
c 128	1214 862	.201 .224	.221 .227	.218
g 192	1412 789	.183 .173	.179 .184	.179
c^1 256	107 86	.162 .166	.158 .168	.163
g^1 384	132 105	.142 .157	.152 .166	.154
c^2 512	141 114	.143 .137	.108 .134	.131
g^2 768	140 101	.098 .108	.112 .105	.105
c^3 1024	136 102	.104 .126	.132 .138	.125
g^3 1536	135 98	.114 .120	.108 .108	.112
c^4 2048	251 139	.077 .081	.082 .101	.085
g^4 3072	332 231	.117 .107	.122 .121	.117
c^5 4096	425 281	.108 .100	.114 .107	.107
g^5 6194	296 219	.160 .157	.155 .145	.154
c^6 8192	260 183	.188 .204	.200 .178	.192
g^6 12,288	580 481	.164 .171	.191 .229	.188

These results are also shown in graphical form. In Fig 21 I have separated the octaves of C from those of G. It will be seen that in both cases sensibility is greatest in the middle range of frequencies and that it falls off fairly sharply at both extremes. The average figures for the tuning-forks are given on the same graph, and show that a great difference exists for the results from the two sources. As regards variation with intensity

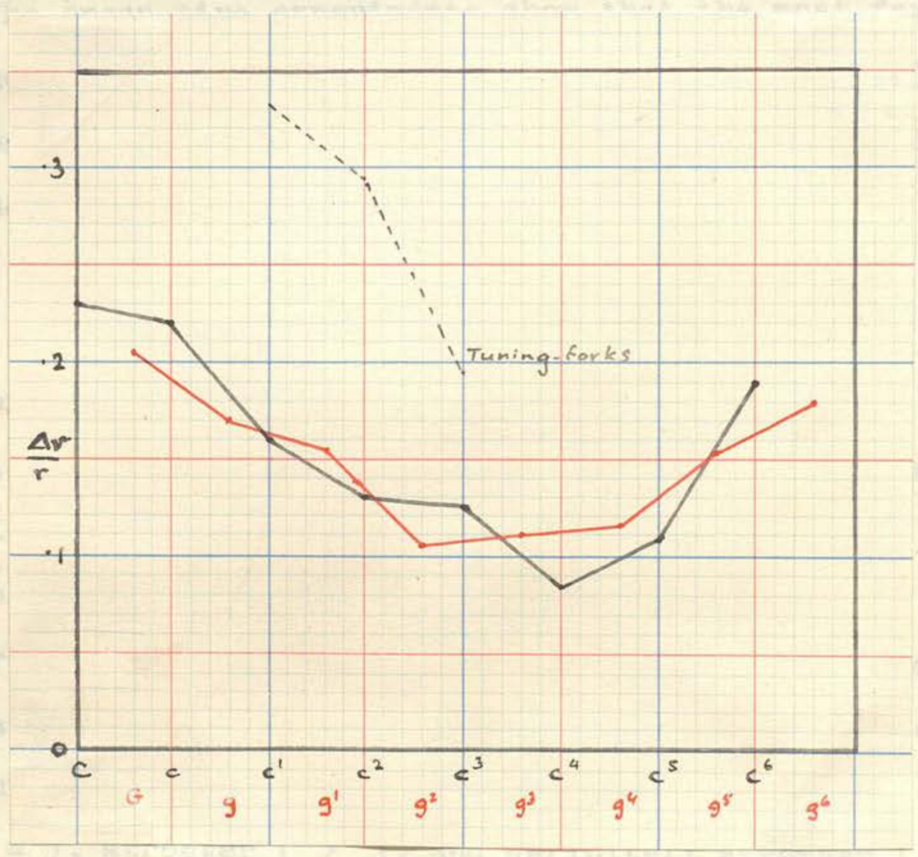


Fig. 21

for the same frequency, no conclusion can be drawn from the pairs of values in Table XIII, as the number of cases of higher and lower threshold-values for each lower intensity are approximately equal.

The conclusions reported by Zwaardemaker are as follows:

(1) From the results of the tuning-fork experiments it follows that Weber's law is valuable in a general way, but that it is not exact for the medium and weak intensities investigated.

(2) The organ-pipe experiments show that the most favourable difference-threshold is found with c^4 (c. 2000 cycles), and that from this point to the extremes, the power of distinguishing differences in intensities decreases rather rapidly.

In 1907 Keller (20) introduced what was virtually a new psychophysical method, that of mehrfachen Fälle. This may be translated 'Method of Manifold Cases' or, better, 'of Manifold Categories'. Like the method used by Mosch (33) it is really an extension of the method of Right and Wrong cases, to which Keller refers as 'der Methode der drei Fälle. Instead of the three categories of the latter, five different categories of judgement are used: definitely weaker ($\ll$), weaker ($<$), equal (=), stronger ($>$), and definitely stronger ($\gg$). Judgements of unentschieden or zweifelhaft were excluded. A full statement of the method of handling the results, is given, but its importance is insufficient to warrant its reproduction here. Experiments were also carried out by the method of limits, and/

and the connection between the two methods investigated.

The apparatus used was Wundt's fall-phonometer: the balls were of ivory. Nine subjects each judged for five standard stimuli, corresponding to heights of 40, 45, 50, 55 and 60 cm. The interval between the variable stimuli was 3 cm. Fifty trials in all were given with each variable. Two subjects worked simultaneously, seated with their backs to the apparatus, at a distance of $2\frac{1}{2}$ metres. Experimental periods consisted of one hour, during which 180 pairs of stimuli were presented. Each began with an easily-judged stimulus. At the end of each group of experiments (a group comprised 5 presentations of each variable along with the standard) the balls used for standard and variable were interchanged to eliminate accidental differences of timbre, etc. Both time-orders were used, and recorded separately.

The results are also represented graphically. A separate figure is used for each standard as judged by each observer, time-orders also being kept separate. Keller prints a selection of six figures; these I have reduced to two. (Figs. 22, and 23).

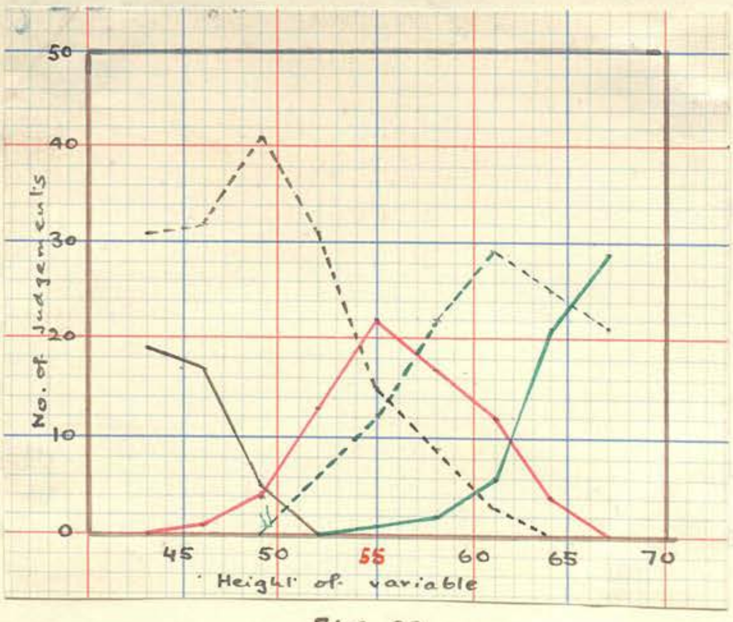


Fig. 22

Subj. Bii Standard 55 Order VS

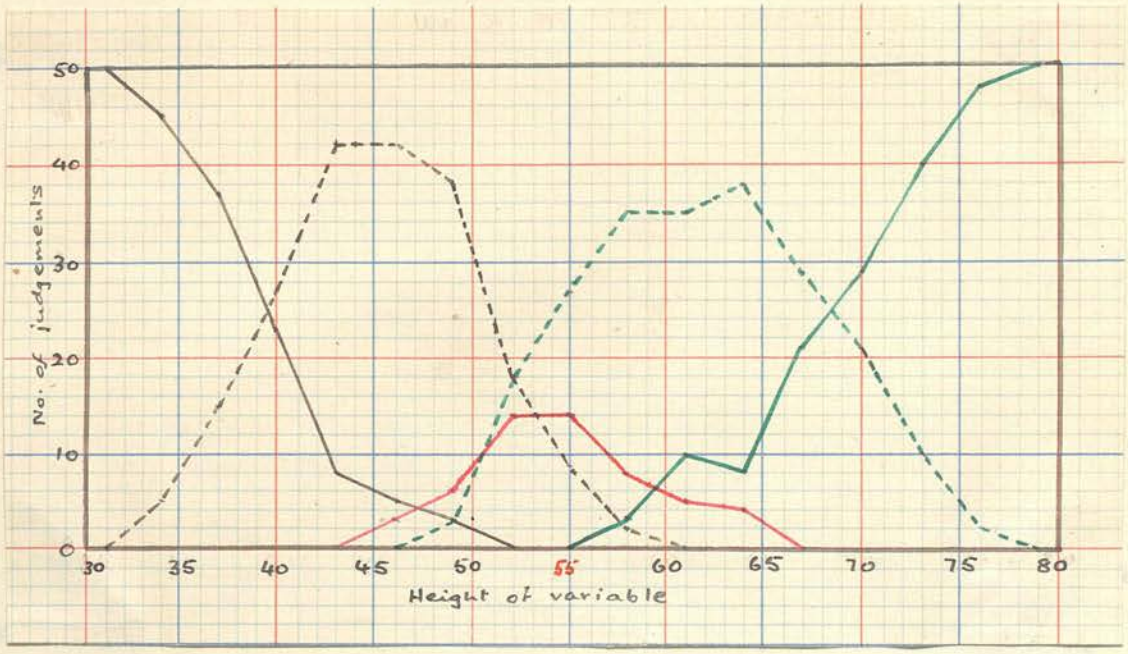


Fig. 23
Subject We Standard 55 Order SV

The abscissae represent stimulus-values as measured by height of fall, the ordinates the number of judgements of each category. The various curves are to be interpreted as follows:

Black line, continuous:		≪
" "	broken:	<
Red line:		=
Green line, broken:		>
" "	continuous:	≫

Considerable individual variation is apparent in these curves, which give a good picture of the observer's peculiarities in respect of distribution of judgements.

Values of the difference threshold were obtained by a consideration of the distribution of (a) the 'equal' cases (b) the 'stronger' and 'weaker' cases, and (c) the 'definitely stronger' and 'definitely weaker' cases. The values are given in Table XIV/

XIV, together with those of the method of limits.

TABLE XIV.

	Threshold.
(a)	1:11 to 1:14
(b)	1:8 to 1:11
(c)	1:20
Limits.	1:8 to 1:10

The experiments with this method were carried out to check the unusually low values of the threshold, as determined by mehrfachen Fälle. Three subjects were used, two of whom judged for both time orders. The stimulus-interval was one second. The figures in Table XV give their results, and show that any

TABLE XV.

Subject:	Bü		Deu	
	Order.		Order.	
Standard.	S V	V S	S V	V S.
40	$\frac{1}{10}$	$\frac{1}{8}$	$\frac{1}{9}$	$\frac{1}{10}$
45	$\frac{1}{8}$	$\frac{1}{9}$	$\frac{1}{10}$	$\frac{1}{9}$
50	$\frac{1}{9}$	$\frac{1}{7}$	$\frac{1}{7}$	$\frac{1}{10}$
55	$\frac{1}{8}$	$\frac{1}{9}$	$\frac{1}{10}$	$\frac{1}{10}$
60	$\frac{1}{10}$	$\frac{1}{8}$	$\frac{1}{10}$	$\frac{1}{10}$

apparent fluctuation is quite irregular, and does not seem to depend on time-order or intensity.

Keller's general conclusions may be summed up as follows:

(1) The values of the threshold, so much lower than those of other investigators, are probably due to improved technique.

(2) Weber's law seems to hold good.

(3) The method of 'Manifold cases' is useful as a connecting link between those of limits and of right and wrong cases.

(4) The Gaussian law does not hold in psychophysical judgements, therefore methods involving its use should be avoided. This conclusion seems to the author to be warranted in spite of the small number of cases on which it is based, and of the lack of practice of the subjects in giving the required types of judgements.

Norton (35) studied the correlation of pitch and intensity discrimination. Pitch discrimination was studied with two instruments: (a) small forks on a resonator (b) a sonomoter or monochord. Intensity discrimination was studied with a sound pendulum, standard 90°, so presumably only a lower threshold was found. In both cases the method of limits was used, but with unequal intervals. The rather curious statement is made that in the case of intensity the variables differed from the standard 'by just twice the quantities used for pitch, but expressed in degrees'. The difference thresholds for intensity are not given.
(i represents intensity discrimination)
The correlations found were as follows:

TABLE XVI.

	Pearson	Median ratio	Modal ratio
r_{ab}	.75	.89	c. 1.00
r_{ai}	.30	.45	
r_{bi}	.39	.67	.45

The pitch tests are shown to be reliable, while, to quote the author 'the validity of the intensity tests awaits further knowledge of the reliability of the falling pendulum'.

Accordingly, the only conclusion possible is that intensity discrimination appears to have some connection with pitch discrimination. The lack of P.E.'s prevents us from saying with what degree of certainty. No actual data bearing on Weber's law are given.

Rich (213) makes a passing reference to intensity discrimination. In a discussion of the difference threshold for tonal volume he states that this is 'fairly constant for the whole range . . . as nearly constant as the ordinary limen for intensity under Weber's law'. The implications of this statement are not very obvious, but it does not seem probable that Rich carried out experiments on intensity. However, the reference is useful as showing at least a partial acceptance of Weber's law as applied to sound intensity.

Guernsey/

Guernsey (13), in an article devoted chiefly to liminal sound intensities, deals also with the application of Weber's law to tones of different pitch. Her work is important as one of the earliest to apply electrical apparatus to problems of sound intensity. Her method consisted in essence of a determination of the amplitude of vibration of a telephone plate for some known current strength.

The subject was seated in a sound-proof room, the distance between ear and telephone-plate being kept constant by means of a head and mouth rest. Reactions were transmitted to the apparatus room by a telegraph key, using a simple code of signals.

Some preliminary work on Weber's law was done with a constant stimulus of 120 cycles. Intensity was graded by two methods: (a) using a sliding rheostat, and (b) by connecting the telephone in shunt with a Leeds-Northup resistance-box. The following results show the average of six subjects:

	Threshold.	P.E.
(a)	.2844	.069
(b)	.3152	.0422

In the main Weber's law experiments, fourteen just noticeable intensity steps were determined for each of the subjects, starting from the absolute threshold. The tone arrived at by summating these steps was shown to be about as loud as the ordinary speaking voice. In Table XVII the average of these fourteen difference thresholds is shown for each of three of the six subjects in the columns headed F (for 'Fraction'). The column headed L (Limen) gives the/

TABLE XVII.

Rate	PALMER L	PALMER F	PALMER P.E.	PAGE L	PAGE F	PAGE P.E.*	MIBAI L	MIBAI F	MIBAI P.E.
120	5.10^{-9}	.415	.042	8.10^{-8}	.396	.031	2.10^{-7}	.433	.011
384	4.10^{-13}	.279	.085	9.10^{-12}	.298	.074	5.10^{-10}	.401	.125
512	5.10^{-13}	.260	.028	3.10^{-11}	.304	.031	3.10^{-10}	.418	.083
960	3.10^{-12}	.303	.008	2.10^{-12}	.205				
1706 2/3	1.10^{-13}	.209	.066	5.10^{-12}	.293	.104			
2048	2.10^{-13}	.196	.071	4.10^{-12}	.290	.081	1.10^{-12}	.301	.086
2560	6.10^{-14}	.143	.106	9.10^{-13}	.286	.008			
3413½	7.10^{-15}	.273	.030	5.10^{-14}	.183	.092	3.10^{-12}	.215	.042
3840	8.10^{-15}	.202	.046	3.10^{-13}	.265		7.10^{-13}	.297	.064
4096	4.10^{-15}	.178	.053	7.10^{-14}	.201		1.10^{-12}	.268	.072
6400	1.10^{-14}	.146	.114	8.10^{-14}	.205	.050	3.10^{-13}	.274	.084
9216	3.10^{-13}	.304	.091	6.10^{-13}	.495	.137			
12288	9.10^{-11}	.528	.173	2.10^{-12}	.563	.101	3.10^{-11}	.674	.017
13650 2/3	7.10^{-11}	.479	.092	8.10^{-9}	.837	.209			

* Blanks have been left in this column where the values of P.E. given are of an order about ten times that of the others. I cannot help thinking that these are misprints in the original.

the absolute threshold measured in ergs. The Probable Errors of F are also noted. I have also given the values of the difference thresholds in Fig. 24. From this figure it will be seen that the

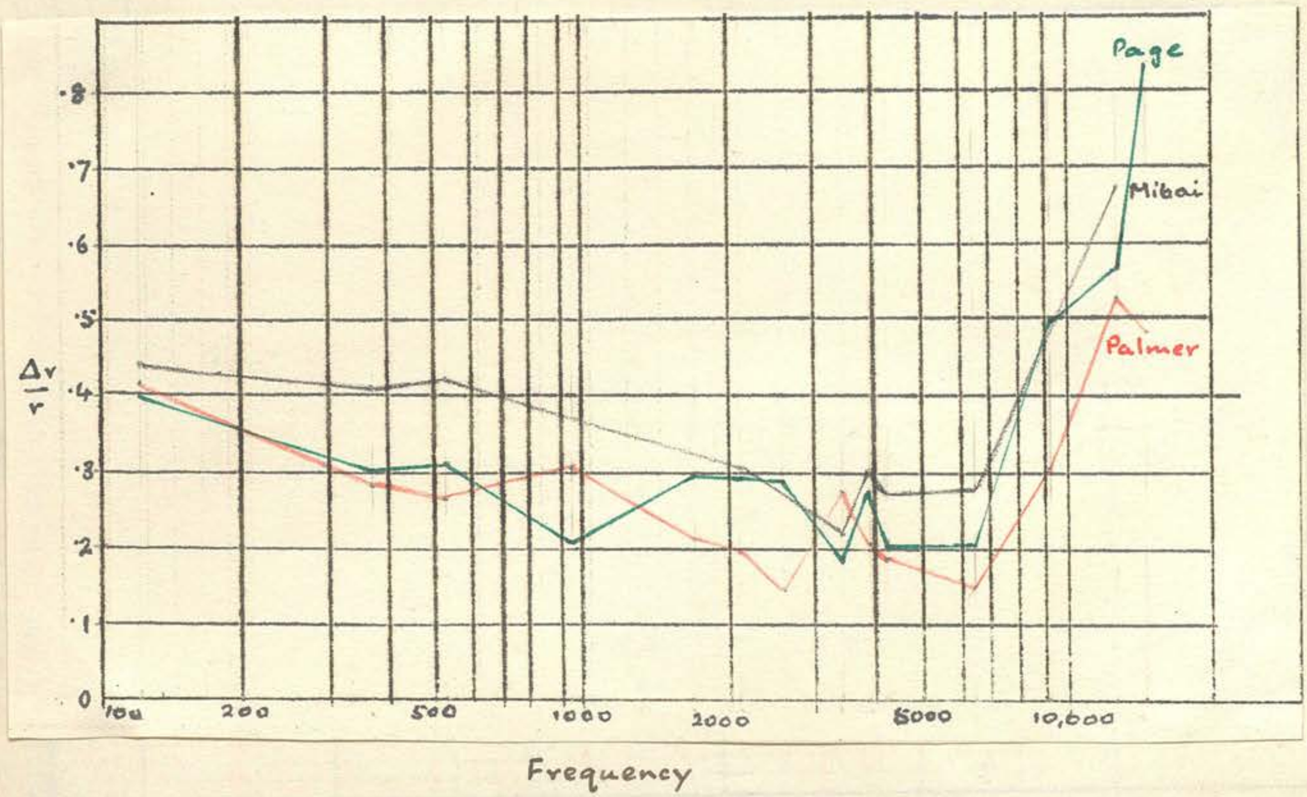


Fig. 24

difference threshold tends to fall with frequency, to reach a minimum about the region 3000 to 7000 cycles, and thereafter to rise rather rapidly. The curves of all three subjects follow nearly the same course, and there is little overlapping.

Guernsey also gives a table of the actual values of the fourteen steps for eight frequencies (one subject only). I have given curves for three of these frequencies in Fig. 25. It will be seen that the variation is rather irregular, except for a decided drop when the intensities/

intensities nearest the absolute threshold are passed (about the third, fourth, or fifth step). This agrees well with the results of other investigators. The values given are for an ascending

Fig. 25

series of intensities, i.e., starting from the absolute threshold and working up. A descending series gave slightly higher results. This, it is noted, is in contrast to the results of Smith and Bartlett (232). (In Table XVII both series are averaged).

The general conclusion is that Weber's law holds 'rather consistently' for sound, within certain limits, although the fraction is identical in no two instances. This discrepancy is put down to errors of observation. A general value of $\frac{1}{3}$ is taken to/

to represent average sensibility, and this increases to $\frac{1}{2}$ near the absolute threshold. The value is also higher for very high and very low frequencies.

In 1922, the year of publication of Guernsey's article, a research embodying a new principle was published by Mackenzie (29; short version 29a). This may be taken as a precursor of the loudness-balance work described later, although Mackenzie's point of view is that of the physicist rather than of the engineer.

Mackenzie makes a brief criticism of the method of making loudness balances by simply changing intensities until the sounds of different frequency under observation are accepted as equally loud. Presumably, he says, different mechanisms respond to the different frequencies f_1 and f_2, so that a comparison is made between a 'decayed' response to f_1 and a 'fresh' response to f_2. To combat this difficulty it is necessary to use some such device as the 'Alternation phonometer' described by Mackenzie. This involves a phenomenon analogous to optical 'flicker'. If two frequencies alternate in the ear at a suitable (fairly rapid) rate, the interruptions of the louder are more conspicuous than those of the weaker. It is possible to adjust the intensities until the two tones appear equally interrupted. At this point the respective loudnesses may be taken as equal. Mackenzie shows that untrained observers can reproduce their judgements of this equality-point within 4%. Further, if f_1 and f_2 are independently equated in this way with f_3, it is found that f_1 will be equal to f_2.

Examples/

Examples are given of pressure ratios of tones equated by balancing each with a third tone in this way, and it is shown that the calculated values agree with those obtained by direct comparison.

The tones were produced by two vacuum-tube oscillators which supplied current to an electrical generator - either a thermophone, or an electromagnetic receiver.

An extended experiment was carried out with 20 subjects (10 men, 10 women), using ten frequencies between 100 and 4000 cycles, and from intensity levels falling within a range of acoustic pressures in the ratio 10:1.

Starting from the Weber-Fechner equation

$$S = c \log P + a$$

where S represents loudness and P acoustic pressure, balanced loudness can be represented by the equation

$$S = C_1 \log P_1 + a_1 = C_2 \log P_2 + a_2$$

$$\text{or} \quad \log P_1 = \frac{a_2 - a_1}{C_1} + \frac{C_2}{C_1} \log P_2 .$$

Accordingly, if the Weber-Fechner law holds good, Mackenzie's results should be reducible to the form

$$\log P_1 = A + B \log P_2 ,$$

and this Mackenzie shows to be the case in 93 out of the 180 comparisons provided by his subjects. No systematic deviations were observed in the other cases, so the discrepancies were explained as due to experimental blunders. Different subjects gave different values of A and B. A condensed table giving average results for the whole group, balancing each of the frequencies against a reference-tone of 700 cycles is given. (The frequency 700/

700 was chosen as being the last frequency audible when the human voice is progressively attenuated.). The essential part of the table is given below:

TABLE XVIII.

f	A	B
200	.76	.97
300	.52	.98
500	.21	.98
700	.00	1.00
1000	-.21	1.00
1500	-.45	.99
2000	-.60	.89
3000	-.82	.84
4000	-1.14	.74

Since the thermophone can give only one-sixth of the range of loudness within which the ear can be accommodated, an air-damped electromagnetic receiver was used to complete the range. Results in agreement with those of the thermophone were obtained with four subjects. Mackenzie's general conclusion is, therefore, that relative sensibility of the ear is invariable over almost the whole range of hearing, though room noises made work very near the absolute threshold impossible. Individual differences are smaller at intermediate levels than near the extremes. No significant sex-differences were found.

On/

On this survey, then, the Weber-Fechner law is valid.

Knudsen (25) studied the sensibility of the ear to small differences of intensity and frequency. Sensibility in each respect was studied as a function of both loudness and pitch. The values found are designated $\frac{\Delta E}{E}$ (for intensity) and $\frac{\Delta N}{N}$ (for frequency), but Knudsen recommends the use of the inverse, or the logarithm of the inverse, of these.

A short historical survey is given, stressing the work with tones of Wien (45) and Deenik (6).

Knudsen's apparatus was a telephone receiver supplied with current by a vacuum tube oscillator. A frequency range of 30 to 20,000 cycles was obtainable. Intensity could be varied through a complete range by means of a divided and balanced resistance circuit. The wave form was maintained practically sinusoidal, and errors from overtones were eliminated by the use of appropriate resonators. A motor-controlled key periodically changed the resistance across which the receiver was shunted by any desired units, so that the tone periodically and abruptly fluctuated in intensity. It was found best to use two tones of equal duration alternated at a rate of about 50 per minute. Starting from a point at which the differences of intensity were plainly perceptible, the difference was gradually decreased until the original "flutter" tone merged into a steady tone. The process was then repeated in the opposite direction, i.e., the method was similar to that of limits. Judgements were signalled by two signal lamps. With practice/

practice it was possible to work within an error of 10%. At least four observers worked at each intensity.

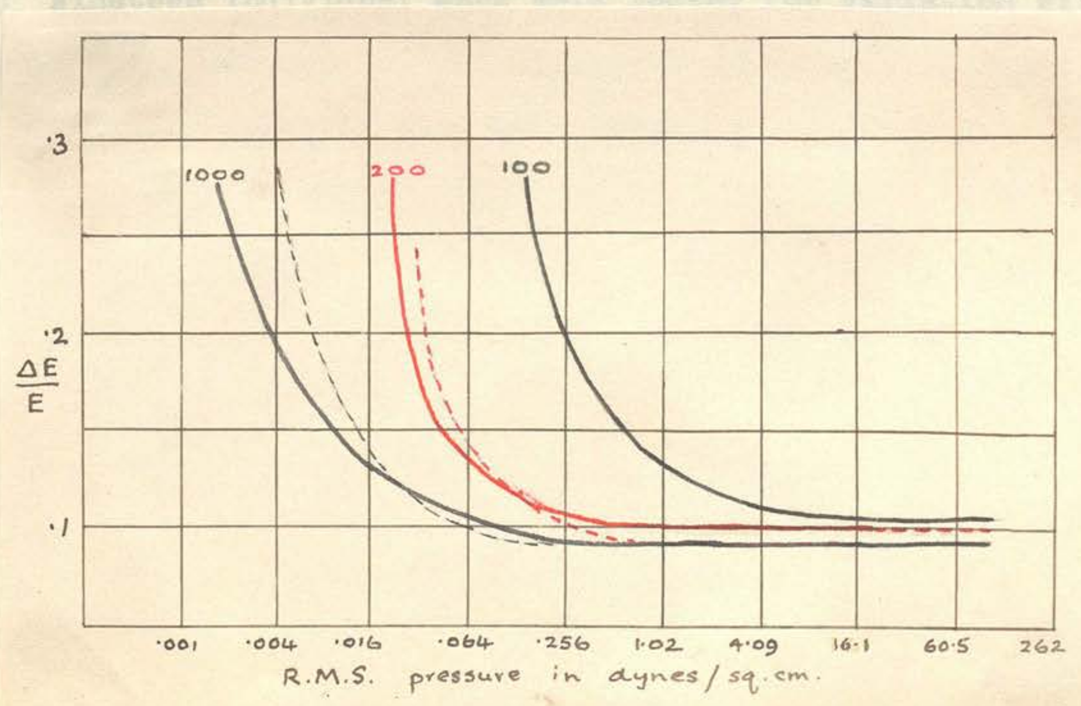
Since it can be shown that there is a linear relationship between the electrical energy actuating the receiver and the acoustical energy developed in the vibrating diaphragm, it is possible to take change of current in the receiver as a measure of the just noticeable difference of the tone. Knudsen shows that differential sensibility may therefore be expressed by the equation

$$\frac{\Delta E}{E} = \left(1 + \frac{\Delta I_R}{I_R + \Delta I_R}\right)^2 - 1$$

where I_R is the current in the receiver.

The majority of Knudsen's results are given in a series of curves. In the printing these have unfortunately been reduced to an extent which makes them almost unintelligible. I have, however, disentangled and redrawn them to the best of my ability. Knudsen's Fig. 3, showing $\Delta E/E$ against a relative scale of intensities defied all efforts, but Fig 26, below, shows Knudsen's Fig. 4, in

Fig. 26



which $\Delta E/E$ is plotted against an absolute scale of intensities derived from the threshold intensities for the relevant frequencies given by Fletcher and Wegel (119). The conclusion for this part of the work is that differential sensibility is some continuous function of intensity, though not such a simple one as the Weber-Fechner law requires. For feeble intensities $\Delta E/E$ is almost inversely proportional to some logarithmic function of intensity; for medium and high intensities the ratio is nearly constant. In other words, for these intensities the Weber-Fechner law is valid - with a value of about $\frac{1}{10}$. From this it follows that under favourable conditions the ear can distinguish about 400 gradations of loudness (in a tone of 1000 cycles), the energy of the loudest being 10^{12} times as great as the energy at the absolute threshold.

Minor problems investigated by Knudsen include the variation of intensity-discrimination as a function of (i) frequency, (ii) quality, and (iii) time-interval.

(i) Nineteen individual ears were tested for variation with frequency. The average for all of these, together with two individual curves, is shown in Fig. 27. Individual differences are very

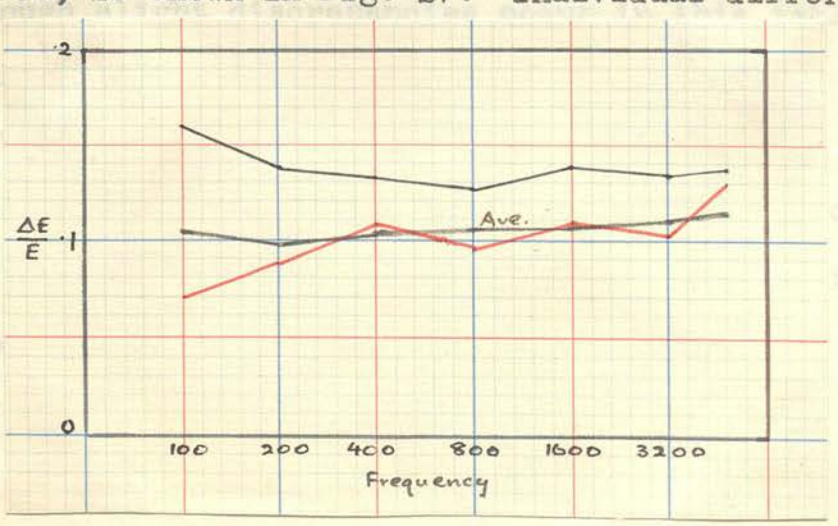


Fig. 27

noticeable, even between the two ears of the same person. On the whole it would appear that discrimination is slightly coarser at the extremes - though to an extent rather less than in the results of other investigators.

In this investigation the subjects were asked to judge only for a difference of intensity, without regard to direction of difference. In order to check for possible errors arising from this cause, five ears were tested by the method of right and wrong cases, (a) with, and (b) without the help of a König resonator. The following values were obtained, the three columns representing respectively Limits, Right and Wrong Cases (a), and Right and Wrong Cases (b).

TABLE XIX.

f	L	(a)	(b)
200	.098	.118	.117
400	.103	.115	.108
800	.105	.113	
1600	.107	.106	

Although slight discrepancies occur in this table, the check may be taken as satisfactory.

(ii) When the quality of the tone was altered by changing the plate resistance of the oscillator circuit, it was found that the degree of purity had little or no effect on sensibility. Tones of 200 and 400 cycles were tested in this way.

(iii)/

(iii) The length of time-interval in the method of right and wrong cases was shown to have quite a considerable effect. Five persons were tested, and all their results are given in Fig. 28. The average curve follows that marked AA very closely.

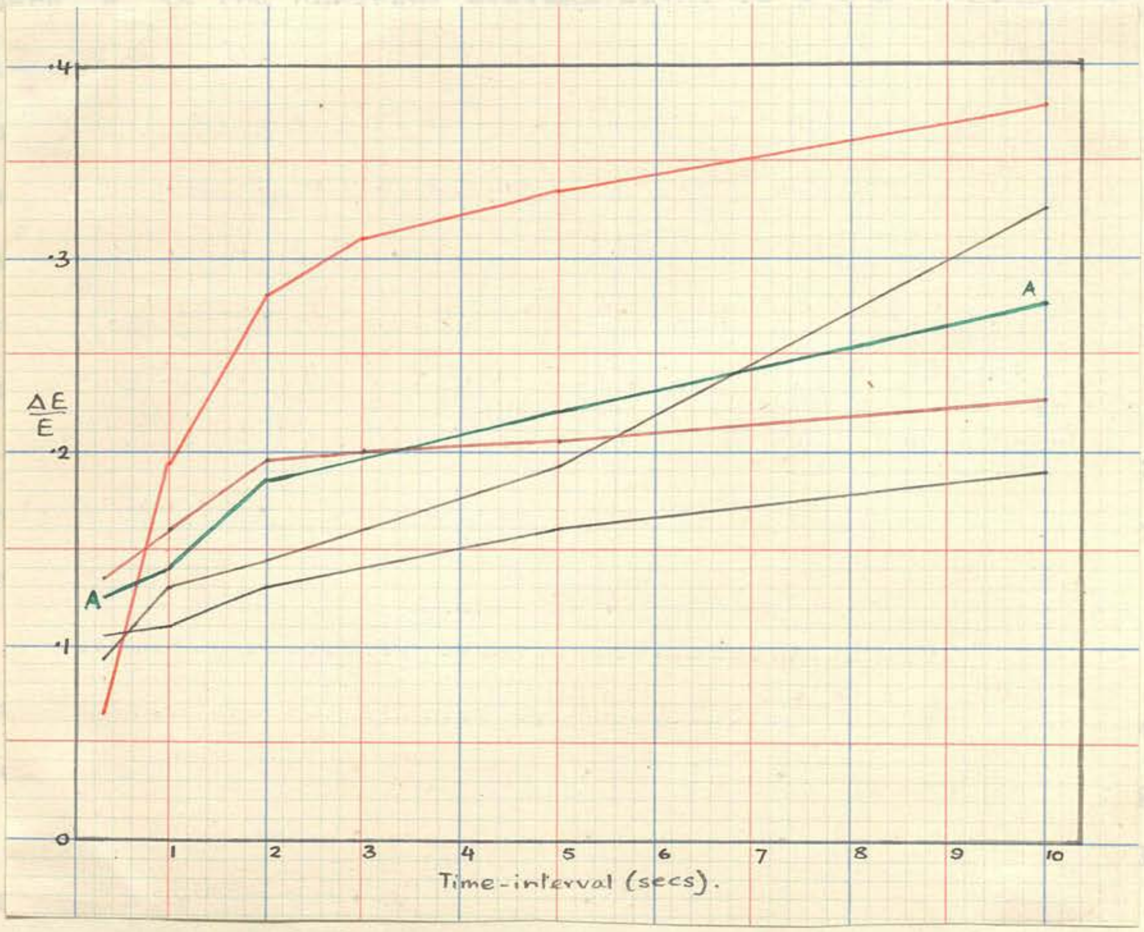


Fig. 28

Apparently there is a fairly steady increase in threshold with increase in time-interval. Knudsen compares the results with what he calls the 'well-known parabolic memory curve'.

In a discussion of his results Knudsen points out the general similarity of his results to those of Wien and Deenik, although on the whole $\Delta E/E$ is here found to be more independent of frequency than in the two earlier researches. Knudsen concludes by/

by proposing a modification of the Weber-Fechner law on the analogy of Nutting's (193) equation for light sensation:

$$\Delta E / E = F + (1 - F)(E_0 / E)^n$$

where F is the constant minimum value of $\Delta E/E$ at higher intensities, E_0 the pressure at the absolute threshold, and n an empirical constant depending on the frequency. This equation can be shown to satisfy terminal conditions, since at the absolute threshold $E_0/E = 1$ and so $\Delta E/E = 1$, while at high intensities E_0/E is very small, and so $\Delta E/E = F$. Theoretical curves for this equation are shown in Fig. 26 by dotted lines. It will be noticed that this equation does not allow for the increased threshold at very high intensities found by many other investigators.

Weber's law is thus claimed to be valid, except near the absolute threshold. Knudsen suggests that curves for other sensation modalities might be found to be similar, and if so it might be possible to formulate a generalized relation between stimulus and sensation.

Halverson (15) studied tonal volume as a function of intensity. His work also contains results on the difference threshold for intensity, with which he compares difference thresholds for volume.

The apparatus consisted of a telephone receiver in the secondary circuit of an audio-oscillator which gave a tone of 1000 cycles. A slide rheostat graduated in millimetres gave alterations in intensity, a reading of zero giving maximal intensity, and/

and one of 233 mm. minimal. Fifteen different readings, ranging from 200 to 0 mm. were used as standard intensities, and five variables were compared with each standard by the method of right and wrong cases. No data are given as to the relative intensities, so it is impossible to calculate fractional thresholds in the usual way. However, Halverson gives a table showing individual and average results for three observers.

In Fig. 29 I have adapted Halverson's graphical representation

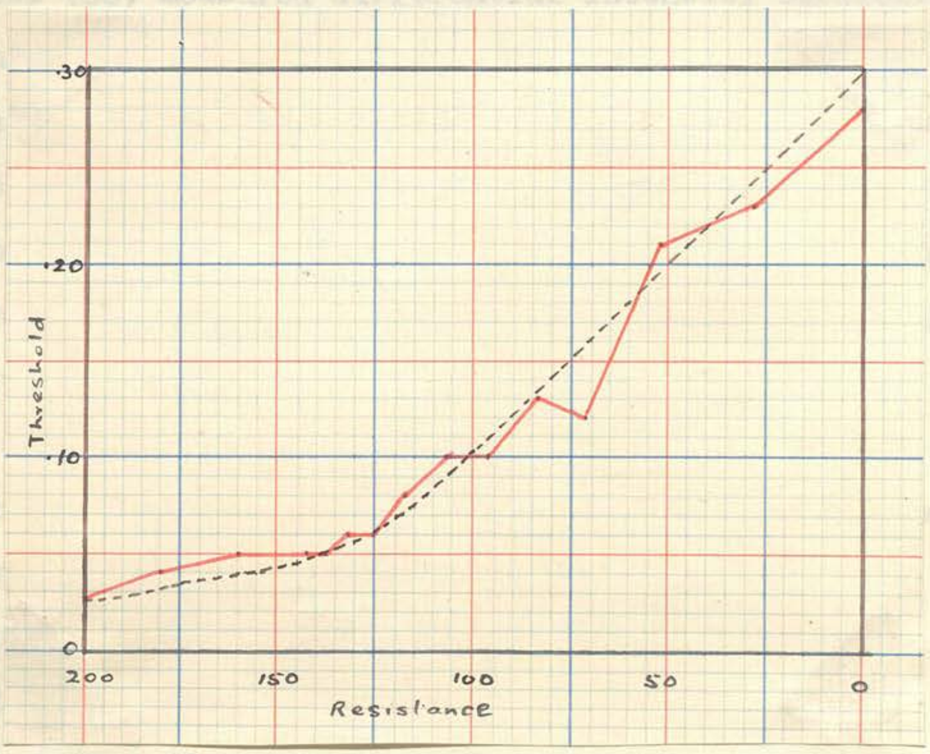


Fig. 29.

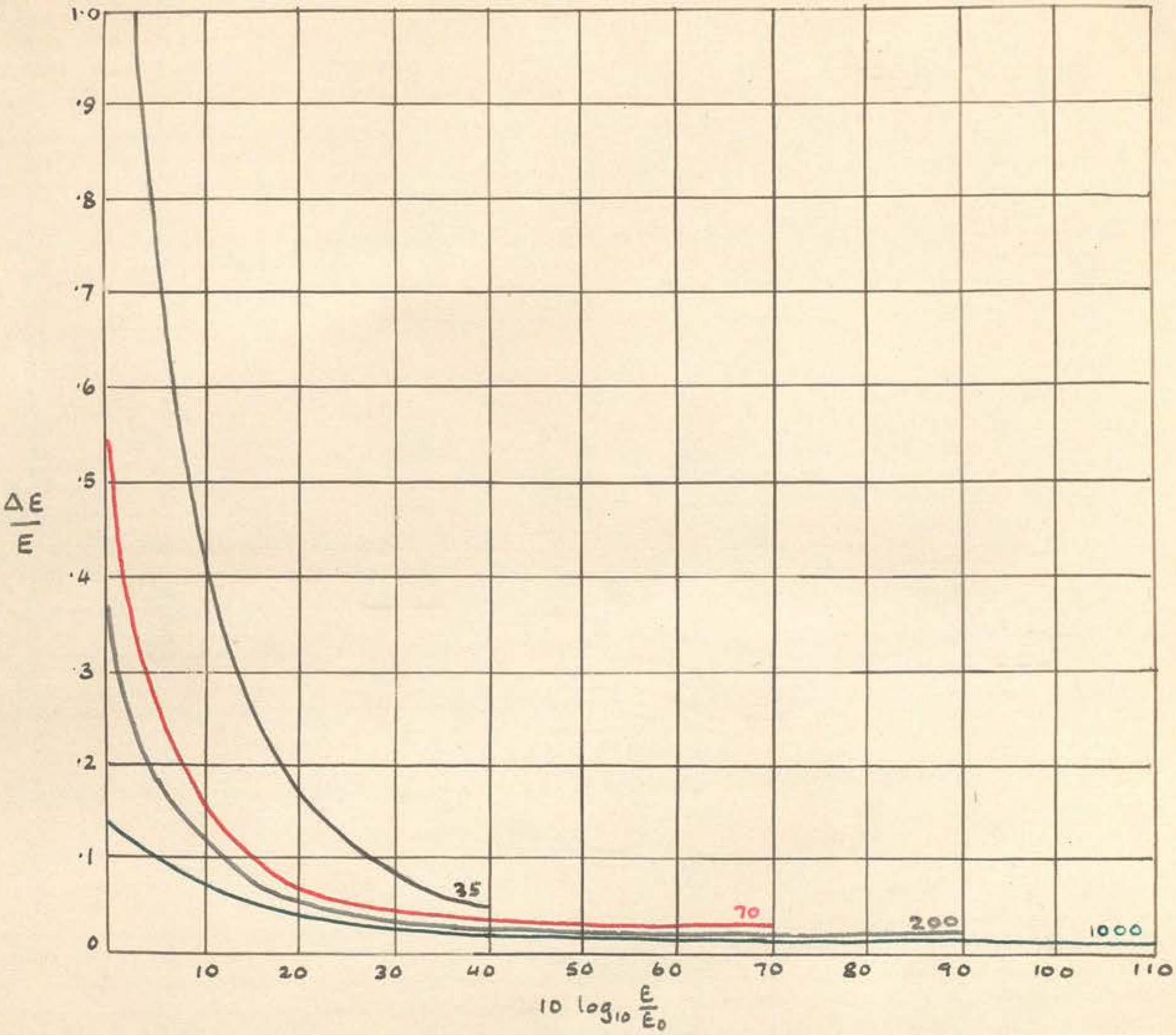
of results so that the abscissae represent increasing intensities from left to right, according to the usual practice. A Weber's law curve would be a straight line of constant gradient, such as is actually seen in the dotted line between the values 100 and 0. This/

This dotted line represents a smoothed curve for the average observed data; the actual curve is given by the red line. Halverson shows that in the range mentioned Weber's law holds good at a fraction of about $\frac{1}{5}$. (This, incidentally, represents the highest value found using an oscillator apparatus). The lower part of the curve can be shown to indicate decreased sensibility at lower intensities, as found by practically all investigators.

Riesz (36) measured differential intensity sensibility by determining for a tone of given frequency and intensity the minimal intensity to which a second tone differing from the first by 3 cycles per second had to be raised to make the beats just perceptible. The choice of 3 cycles was determined in a preliminary experiment, in which the variation of differential sensibility with various rates of beat (between about .15 and about 40 per second) was studied. It was found that $\Delta E/E$ reached a minimum at about 3.2 beats per second. Twelve observers each worked with seven tones ranging from 35 to 10,000 cycles. The whole range of intensities, from the absolute threshold to near the threshold of feeling was covered.

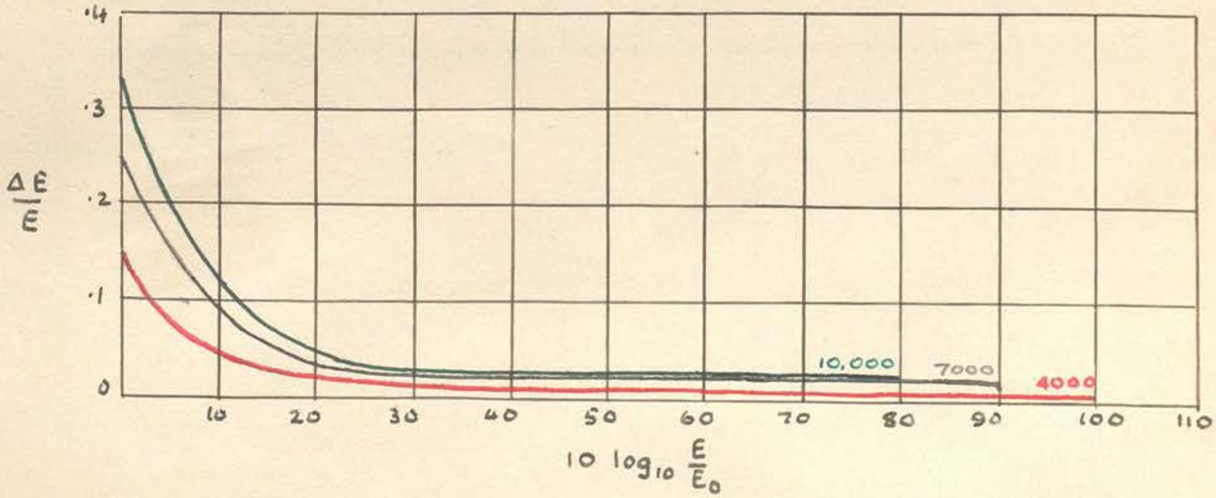
The source of sound was a special moving coil telephone receiver, supplied with alternating current by two vacuum-tube oscillators. An approximation to the method of limits was used.

The complete results for all seven tones are shown graphically in Figs. 30 and 31. They are also given in another form in Fig. 32. In this each curve represents the course of the difference/



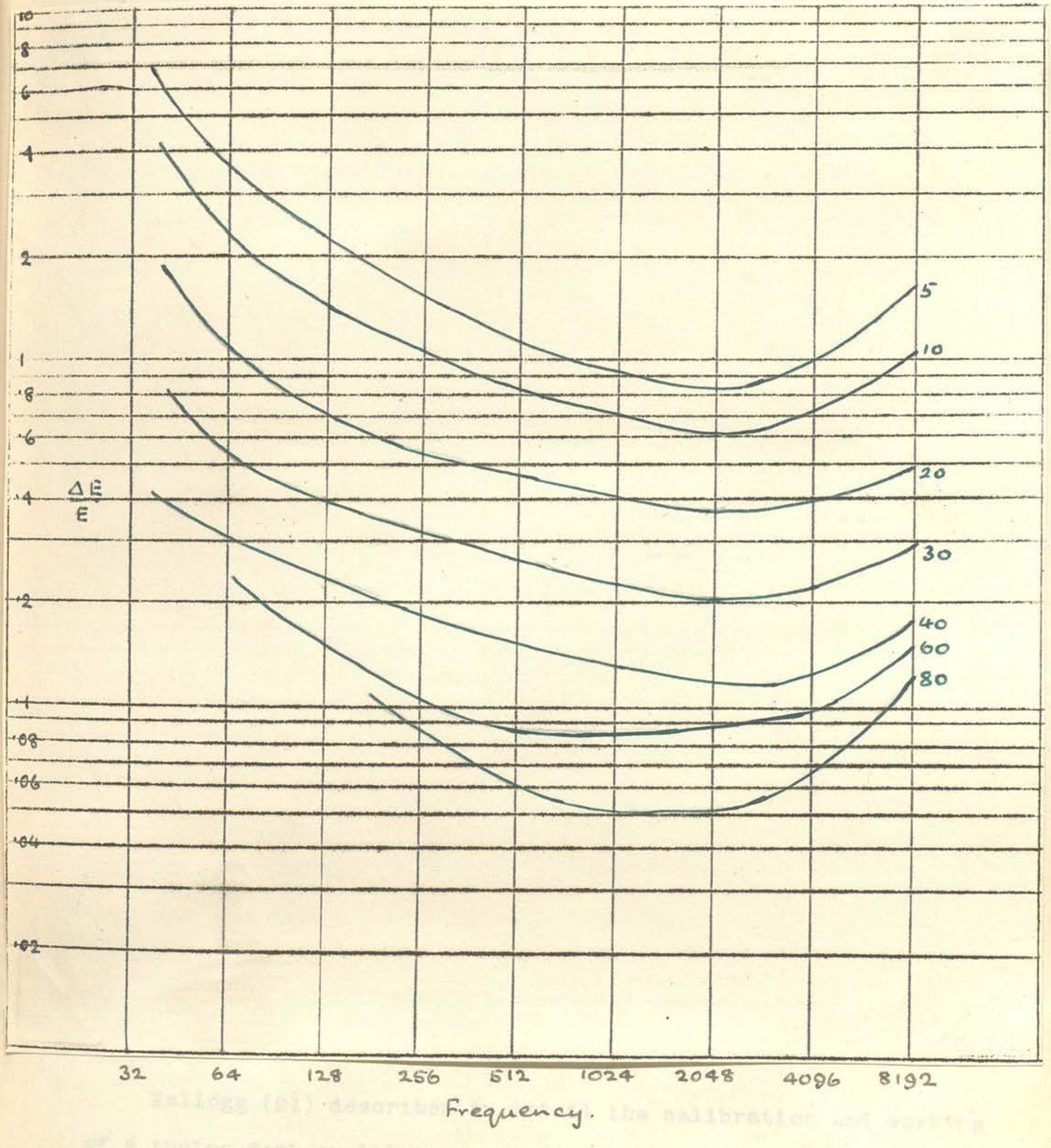
1·0
·9
·8
·7
·6
·5
·4
·3
·2
·1
0
$\frac{\Delta E}{E}$
35
70
200
1000
10
20
30
40
50
60
70
80
90
100
110
$10 \log_{10} \frac{E}{E_0}$

Fig. 30



·4
·3
·2
·1
0
$\frac{\Delta E}{E}$
10,000
7000
4000
10
20
30
40
50
60
70
80
90
100
110
$10 \log_{10} \frac{E}{E_0}$

Fig. 31



10
8
6
4
2
1
·8
·6
·4
·2
·1
·08
·06
·04
·02
ΔE/E
5
10
20
30
40
60
80
32
64
128
256
512
1024
2048
4096
8192
Frequency

Fig. 32

difference threshold for some standard intensity (here given in decibels, though Riesz uses $10 \log_{10} \frac{E}{E_0}$) plotted against frequency. Following Beatty (61), I have given $\Delta E/E$ as a logarithmic plot, as this helps to separate out the curves. It will be seen that the values of $\Delta E/E$ for very low intensities rise to a very much higher value than in any other investigation.

Riesz's general conclusion as regards Weber's law is that it holds for all intensities at intensities above 10^6 times the absolute threshold value. The curves in Figs. 30 and 31 seem to confirm this. On the other hand, these curves are extremely cramped in the critical portion of the range, and Fig. 33, obtained by plotting the intercepts on the frequency abscissae of the curves in Fig. 32 against their intensity level, seems to cast doubt on this. Although these curves are necessarily approximate and incomplete, the figure has the advantage of being expanded in the critical portion of the $\Delta E/E$ scale, and the general shape of the curves certainly makes a sudden flattening rather unexpected. It is a pity that Riesz's article, like many others in the Physical Review, does not contain actual tables of numerical data to supplement the diagrams.

Riesz concludes by comparing his results with those of Knudsen, to which they bear a close general resemblance.

Kellogg (21) describes in detail the calibration and working of a tuning-fork oscillator, originally described by Halverson (139). The aim of the work was to standardize a method using a less expensive instrument and less complicated set-up than the use of vacuum-tube/

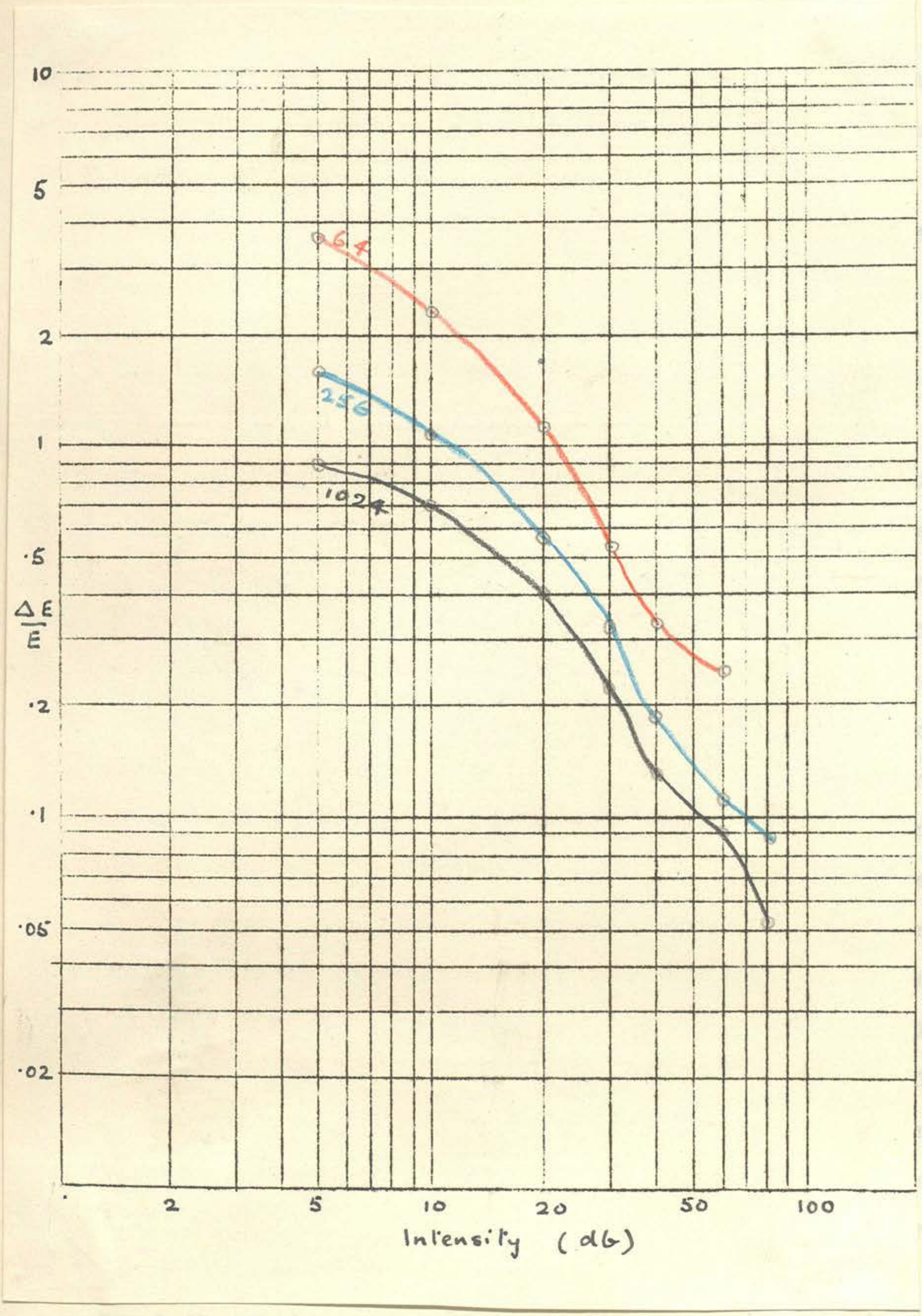


Fig. 33
(adapted from Riesz).

vacuum-tube oscillators entails. The greater part of the article is taken up with a description and justification of the technique employed.

In brief, the tone produced was one of 1000 cycles, in which no partials could be detected. The tuning-fork was screened in a sound-proof box, so that only the receiver tone was audible. The chief original feature of the apparatus was a revolving drum covered with paraffin-paper perforated at intervals to allow contacts with the drum surface which governed the duration and order of the various sound stimuli required. Several such stencils were prepared according to different patterns suited to the requirements of the various psychophysical methods. A rheostat scale graduated in half-millimetres gave accurate readings in arbitrary linear units. The validity of the method of measurement in electrical units rests upon the fundamental assumption that the sound intensity emitted by the receiver (here a Stoelting watch-case ear-phone) is related linearly to the voltage required to produce the sound. This Kellogg shows to be almost certainly true. Equations are given for converting centimetre scale-readings into millivolts, which can be taken as intensity units. The non-linear relationship between voltage and scale-readings shown by these equations may, as Kellogg says, throw some doubt upon the work of several investigators, who, in calculating thresholds in terms of rheostat settings, have tacitly assumed these to be linearly related to sound intensities.

Rough threshold values are given for two intensity levels, apparently/

apparently in the ratio of about 1 : 3 . For the lower the difference threshold is between .056 and .112; for the higher, between .047 and .094.

About the same time as the article just described, Kellogg (22) published 'An experimental comparison of psychophysical methods'. This deals almost exclusively with the Constant method and that of Mean Error, with special reference to the question of which is preferable for a study of Weber's law. Kellogg discusses the classical literature on psychophysical methods at some length, and comes to the conclusion that the variable error of the method of mean error may be taken as proportional to the difference threshold.

In his sound experiments described in this paper, Kelloggused an audio-oscillator giving a tone of 1000 cycles in a telephone receiver. The time-unit was .5 sec. for both stimulus and interval, with 2.5 secs. between each pair. In all cases the standard stimulus came first. In spite of all precautions, fatigue was experienced even in experimental periods of 15 to 20 minutes. Other subjective conditions are also noted, the most important being a marked perseverative tendency to be influenced, in the method of mean error, by the immediately preceding settings.

Five normal trained subjects were used (4 male, 1 female), and each carried out 13 to 18 sittings of one to three hours each. (These sittings included an equal number of Light and Sound experiments). No 'equal' or 'doubtful' judgements were allowed in the Constant method, and no 'reversals ' (i.e. returning to a value already passed) in Mean Error. Responses were always given by depressing/

depressing one of two keys.

The results given fall into two parts:

(i) Theoretical considerations relating to a comparison of the methods

(ii) A number of rough 'sensitivity fractions'. The latter were briefly as follows (all are obtained by dividing the variable error by the standard stimulus):

The 'sensitivity fraction' for sound varies on the average between about $\frac{1}{5}$ and $\frac{1}{20}$. Extreme limits, depending on the observer, the method, and the stage of practice ranged round $\frac{1}{4}$ and $\frac{1}{25}$. The overall average may be taken as about $\frac{1}{8}$. This, Kellogg says, is confirmed by recent work of electrical engineers using modern apparatus. No specific information as to variation of sensibility with intensity is given, but it is obvious that Kellogg's results point to widespread instability.

Some of the conclusions under heading (i) are given in Section VIII.

The work of Macdonald and Allen (28 and 1) represents one of the few sound-intensity investigations of recent years which have not utilized electrical apparatus. The instrument here used was that of Weinberg and Allen (273). It consisted of a Stern variator sounded by a stream of air previously collected over water in a constant-pressure tank. The frequency chosen was 180 cycles. The intensities used were within the range of intensities shown by Love and Dawson (175) to be proportional to the blowing pressure of the air, and hence to the weights placed on the tank. The subject listened/

listened to a standard sound for 2 seconds, after which a small additional weight was lowered on to the tank by means of a lever, thus producing an alteration in the intensity of the sound. 'By repeated trials' a weight was found which gave a just perceptible increment in intensity.

The results of the first series of experiments are given in Fig. 34. The dots of different colours represent determinations

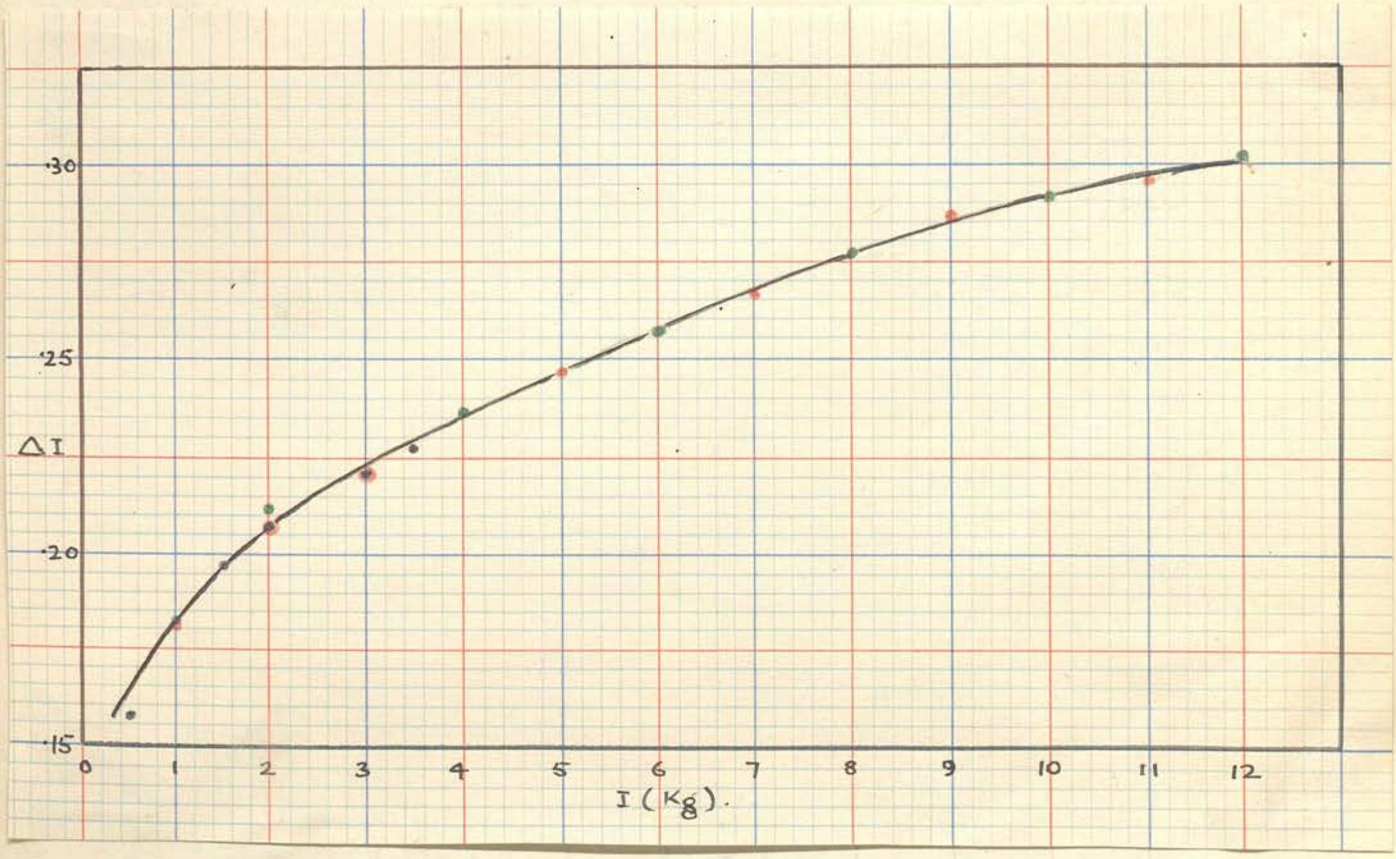


Fig. 34

during different experimental periods. It will be noticed that no serious deviation was found between the results given during these periods, which were, respectively, Sept. 14th A.M., (black) the same day, P.M., (green) and Sept. 15th A.M., (red). Weber's law would require for this curve a straight line of constant gradient, and/

and this is seen not to be so. The form of the curve, however, suggested that a logarithmic relation might exist between ΔI and I. Accordingly reciprocals of ΔI were plotted against log I, and the resulting curve was found to be a straight line, in two parts of different slope. I have not given this curve, since a similar curve for the second series of experiments shows the same properties more clearly.

Since it was considered doubtful whether the weights could legitimately be used as a measure of intensity, another variator tone of 575 cycles and a wider range of intensities were used and calibrated by means of a Rayleigh disc. It was found that the deflections of the disc were not quite proportional to the weights on the tank, but it was found at the same time that the measures of ΔI could still be given in Kg., without any appreciable error. Since the authors do this throughout the papers it is difficult to find values of $\Delta I / I$.

Fig. 35 shows ΔI plotted against I for the second series

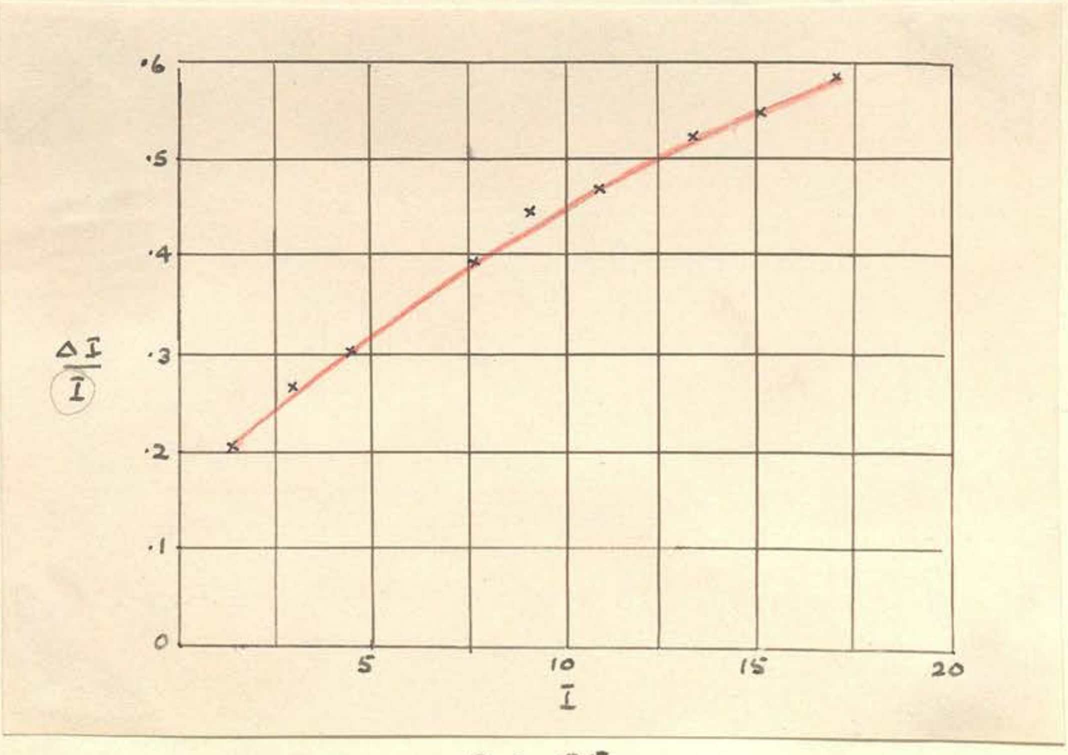


Fig. 35

of experiments, and it is again seen that the ratio is not constant. Fig. 36 shows the new plot: $\frac{I}{\Delta I}$ against log I, and the same 'double straight line' relation is seen to hold. The middle curve represents normal sensitivity; the significance of the others will be stated presently. Macdonald and Allen show that the curve has

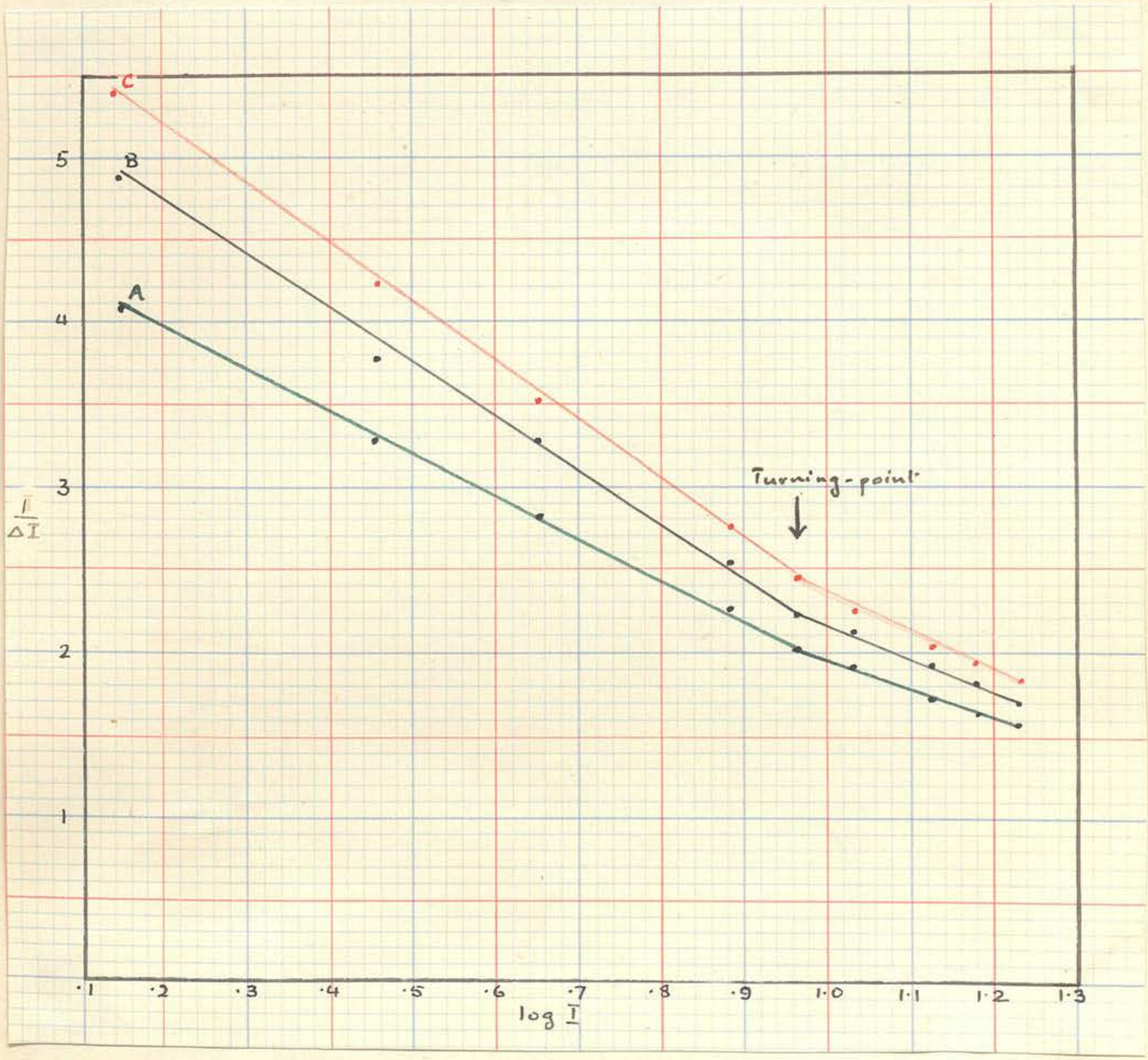


Fig. 36

the equation

$$\frac{1}{\Delta I} = -k^2 \log I + C$$

$$\text{or} \quad \frac{\frac{1}{\Delta I}}{\log I} = C_1$$

where k^2 , C , and C_1 are constants whose values differ for the two parts of the curve. Incidentally, the dividing point between the two parts practically coincides with a similar turning-point in the calibration curve showing the relation between weights on tank and deflection of disc.

TABLE XX.

Wt. (Kg.)	I. (cm.)	log I	Normal A		Depressed B		Enhanced C	
			I	$\frac{1}{\Delta I}$	I	$\frac{1}{\Delta I}$	I	$\frac{1}{\Delta I}$
4	1.4	0.146	.205	4.88	.245	4.08	.185	5.41
6	3.0	0.477	.265	3.77	.305	3.28	.235	4.26
8	4.5	0.653	.305	3.28	.355	2.82	.285	3.51
12	7.6	0.881	.395	2.53	.445	2.25	.365	2.74
14	9.2	0.964	.445	2.25	.495	2.02	.405	2.47
16	10.8	1.033	.475	2.11	.525	1.91	.445	2.25
20	13.3	1.124	.525	1.91	.575	1.74	.495	2.02
24	15.1	1.179	.555	1.80	.605	1.65	.515	1.94
28	17.0	1.230	.585	1.71	.635	1.58	.545	1.84

In the second of the papers under discussion Allen (1) studied depressed and enhanced sensitivity. Allen quotes a number of sources showing that these modifications of sensitivity can be produced/

produced in most sense-departments as a result of stimulation. Weinberg and Allen (273) found that depressed sensitivity often occurred, but they did not obtain enhanced sensitivity. These changes are shown to fluctuate in a regular manner, and are explained as due to the oscillatory character of the neural reactions accompanying sensation.

States of depression and enhancement were obtained by stimulating with a tone of the same frequency, and about 15-20 times the intensity of the test tone. The stimulating tone was given for one minute, after which a test of the difference threshold was immediately taken, and shown to have a value higher than the normal. After a rest of 10 minutes, it was found that the value was now less than normal, i.e., sensitivity had been enhanced. Two varieties of stimulation were given: (a) ipsilateral - stimulation applied to the ear to be tested, and (b) contralateral - stimulation applied to the other ear, the test ear being protected by a heavy pad of wool and metal.

The results given in Fig. 36 and Table XX represent ipsilateral stimulation. Those for contralateral stimulation are similar, but show less displacement from the normal.

The general conclusions are that Weber's law does not hold for audition, but that the equation given above holds for hearing under all conditions. The convergence of the curves in Fig. 36 suggests that at very high intensities enhancement and depression would not occur.

A/

A constructive contribution to the study of Weber's law for sound intensity was made by Kenneth and Thouless (23) who claim to show that the absolute and differential thresholds are continuous with one another. In other words, if $\Delta\gamma$ is a just noticeable difference between two stimulus values γ_1 and γ_2, the absolute value of $\Delta\gamma$ varies continuously over the entire range, down to the threshold of audibility, at which point γ_1 is zero, and so $\dfrac{\Delta\gamma}{\gamma_1} = \infty$ and $\dfrac{\Delta\gamma}{\gamma_2} = 1$. The absolute threshold thus represents a limiting value of either of these ratios, which show continuous variation of some sort, until a point is reached, where, it so happens, the requirements of Weber's law are approximately fulfilled over a certain range. Throughout their article the authors use the value $\dfrac{\Delta\gamma}{\gamma_2}$ (i.e. a 'lower' threshold) since, as they say, it is easier to show a curve falling from unity than from infinity.

The apparatus used was a vacuum-tube oscillator, giving a tone of 724 cycles in a loud-speaker attachment to a gramophone. The general mode of presentation was γ_1 (the lower intensity) against a background of γ_2 (the higher intensity). Two varieties of this presentation-method were used. In essence, method A consisted of two very short γ_1 periods (.08 sec) separated by a slightly longer γ_2 period (.2 sec). Then followed 4.7 seconds of γ_2, while the value of γ_1 was altered in readiness for the next stimulation. In method B the relative periods were much more nearly equal (γ_1 .66 sec ; γ_2 .72 sec) and each pair was presented five times. Method/

Method B was found to give much finer discrimination (see Fig.38) The values of stimulus and threshold are given in milliamps of equivalent direct currents, which may be taken as proportional to intensity. Although no sound-proof room was available, it was demonstrated that extraneous noises had practically no effect.

Fig. 37 shows a curve of variation of the value $\frac{\Delta\gamma}{\gamma_2}$ with

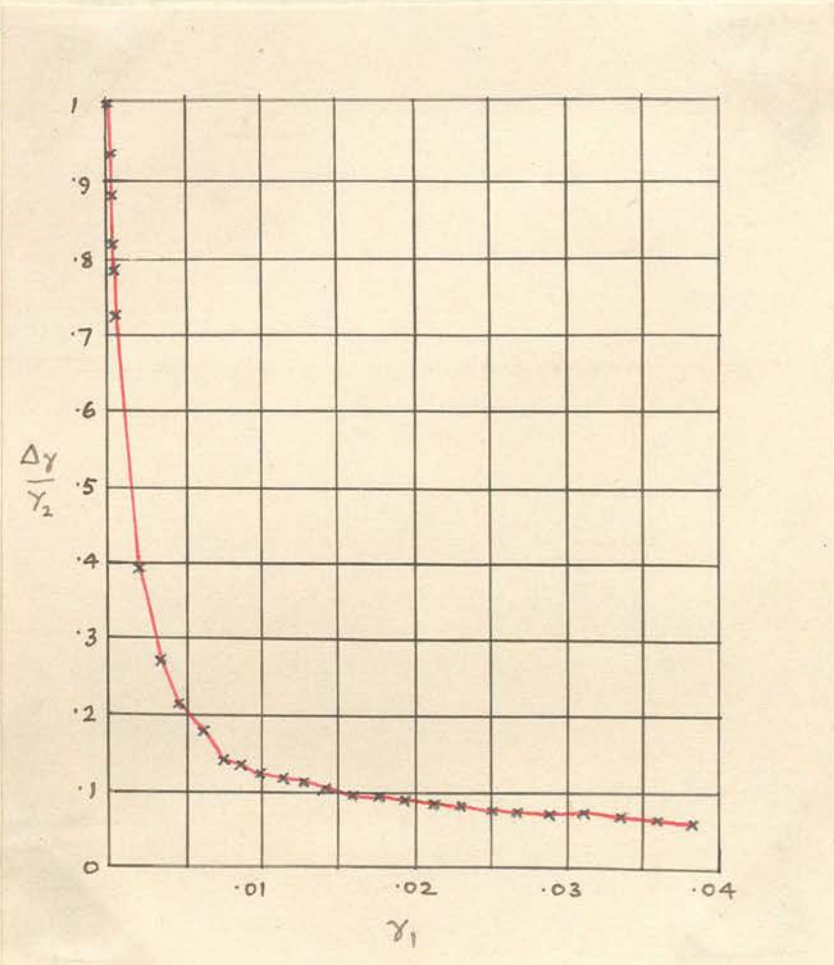


Fig. 37

stimulus strength (of γ_1). The observations for very low intensities were obtained for values of γ_1 starting at zero and taking several further values below the absolute threshold. This curve was obtained by taking the mean of six determinations by method A.

Fig. 38 shows the variation of the just perceptible stimulus

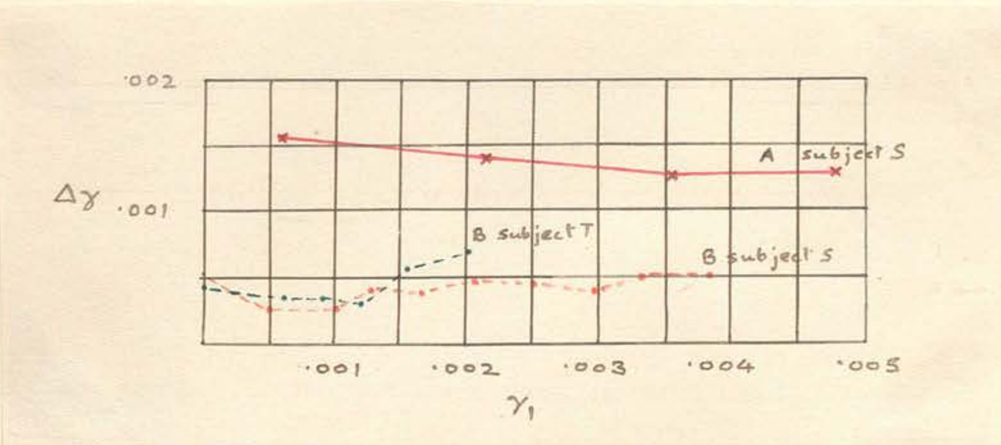


Fig. 38

increment with stimulus strength, and it will be seen that for the low intensities represented the absolute value of $\Delta\gamma$ is practically constant. Both methods, and both the subjects used for the investigation, are represented.

Finally, Fig. 39 shows the data of Fig. replotted, this time with $\Delta\gamma$ against γ_1 . From this it will be seen that the absolute value of the j.n.d. seems to fall at first, and then to rise on a curve/

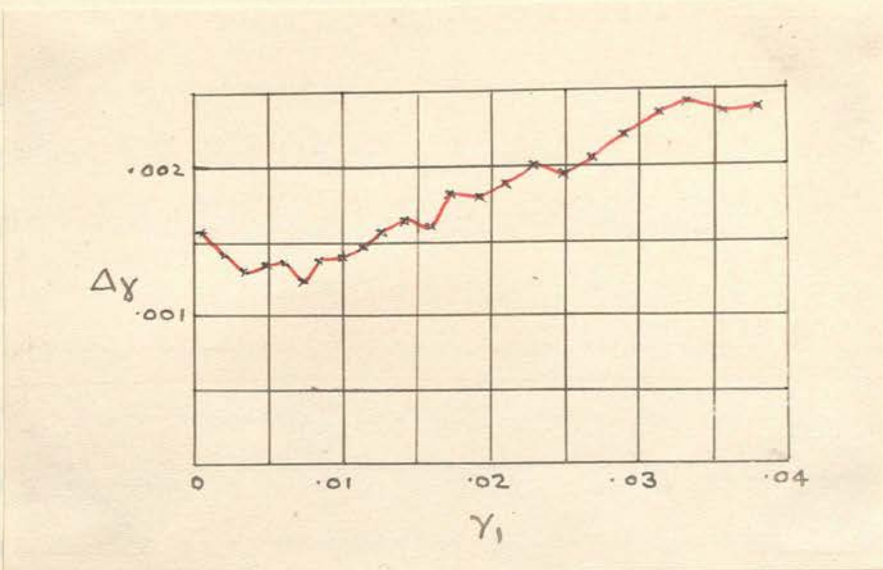


Fig. 39

curve which progressively approximates to the Weber's law straight line of constant gradient.

Gage (11) investigated the variation of the uniaural difference threshold with simultaneous stimulation of the other ear by tones of the same frequency. The chief results are based on curves showing the course of the decibel differential threshold ($D_2 - D_1$) at three intensity levels for each of three frequencies plotted against the decibel difference (B - A) between the tones applied to the stimulated ear (B) and the measuring ear (A). The curves given are smoothed curves, and it is shown that in each case the threshold rises to a maximum as the difference varies from about - 40 db to + 15 or 20 db; then falls to a minimum at about + 35 or 40 db; after this it rises rather sharply. (A negative value here means tone in B less than in A; a positive value/

value - tone in B greater than in A).

The apparatus used was a heterodyne oscillator and resistance capacity coupled amplifier, adjusted to give as pure a tone as possible. Resistances were connected in such a way that two tones differing in intensity could be sounded, alternating at the rate of ten changer per second, i.e. each tone sounded for $\frac{1}{10}$ second. The difference threshold was taken as that difference of intensity which caused the resulting 'flicker' to be only just perceptible. The frequencies and intensities reported by Gage are as follows:

300 cycles	at	34.5, 29.0, and	20.7	db below 1 dyne/sq.cm. R.M.S. pressure.
500	" "	49.1, 47.8, "	40.5	" " " " "
800	" "	40.4, 35,1, "	27.4	" " " " "

It is possible to analyse Gage's results to show how far Weber's law holds under conditions of simultaneous stimulation of the ear not being tested. This I have done as follows:

For each of Gage's frequencies I converted the decibel differential thresholds ($D_2 - D_1$), as shown by the intercepts of the smoothed curves on the abscissae corresponding to B-A values of - 20, 0, 20 and 40 db, into $\frac{\Delta I}{I}$ values. These are given in Table XXI, and are also plotted against intensity level in Fig. 40. In the figure - 20 is represented by dashes, 0 by the lighter line/

TABLE XXI.

B - A	Db below 1 dyme / sq. cm.					
	34.5		29.0		20.7	
Frequency 300	$D_2 - D_1$	I/I	$D_2 - D_1$	I/I	$D_2 - D_1$	I/I
- 20	1.49	.41	.98	.25	.76	.19
0	1.70	.48	1.30	.35	.96	.25
20	2.53	.79	1.40	.38	.91	.23
40	2.13	.63	1.30	.35	1.00	.26
Frequency 500	49.1		47.8		40.5	
- 20	2.45	.76	1.55	.43	1.16	.31
0	3.19	1.08	2.48	.77	1.79	.51
20	4.21	1.64	3.30	1.14	1.54	.80
40	3.27	1.12	2.86	.93	1.82	.52
Frequency 800	40.4		35.1		27.4	
- 20	1.46	.40	1.09	.28	.45	.11
0	2.00	.59	1.32	.36	.86	.22
20	2.10	.62	1.41	.39	.83	.21
40	1.76	.50	1.16	.31	.77	.19

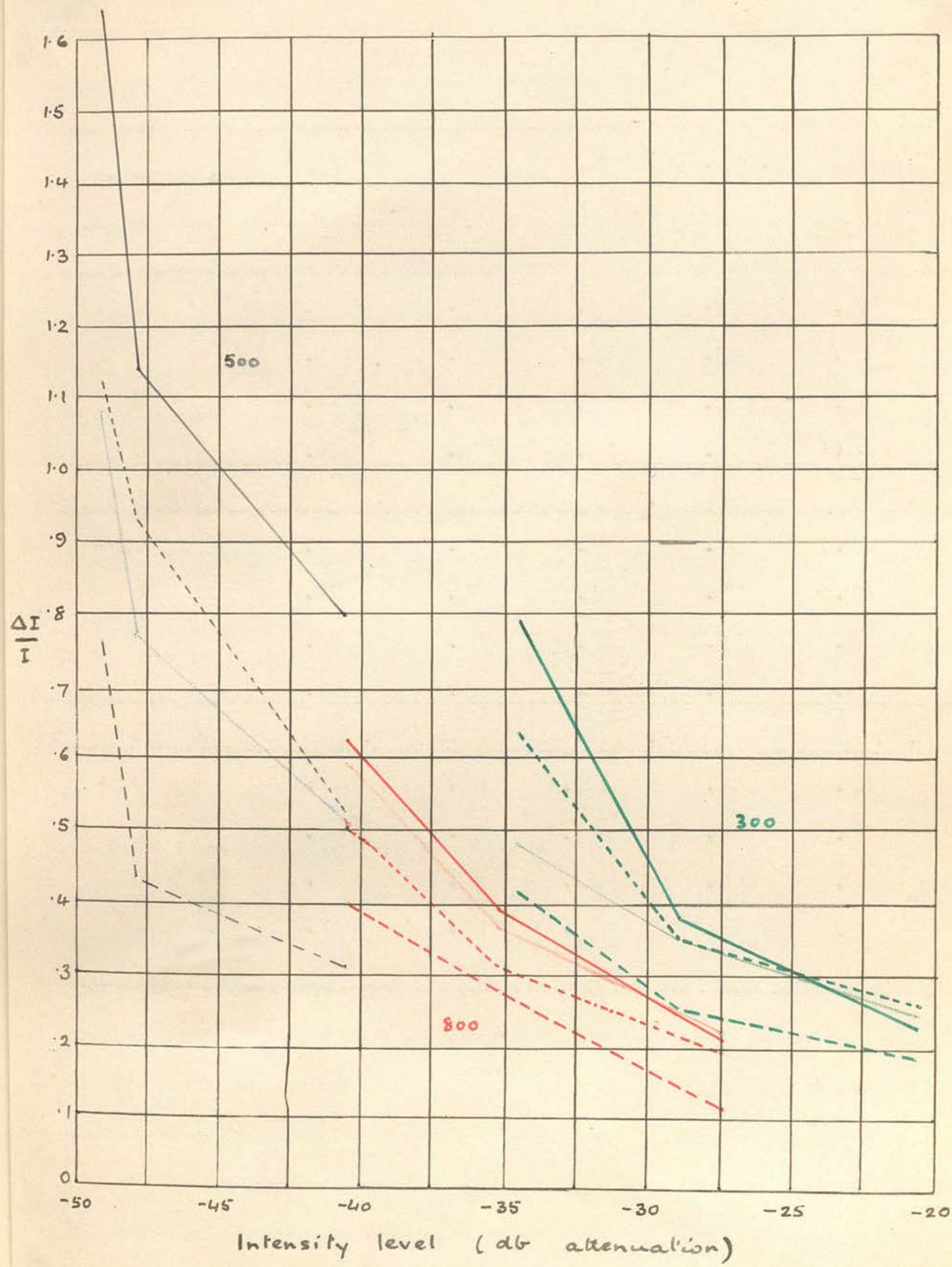

1·6
1·5
1·4
1·3
1·2
1·1
1·0
·9
·8
·7
·6
·5
·4
·3
·2
·1
0
ΔI/I
500
300
800
-50
-45
-40
-35
-30
-25
-20
Intensity level (db attenuation)

Fig. 40

line, 20 by the heavier line, and 40 by dots. It will be seen that in every case sensibility improves with rise in intensity. For very low intensities the value of Δ I/I is seen to be rather high under these conditions. If Gage's conclusion that when the interfering (or 'stimulating') tone is less in intensity than the measurement-tone the former has little effect on the threshold is true, it would seem that Gage's results on the threshold in general suggest similar high values of the threshold at low intensities under normal conditions. Thus the usual 'lower deviation' would be demonstrated as existing in a very marked degree. Gage did not work at sufficient intensity levels to show whether the Δ I/I curve would rise at higher intensities.

The most recent work bearing directly on the validity of Weber's law for sound intensity is that of Telford and Denk (43). Their findings confirm those of previous investigators in most essentials. They also show the incidence, to a fairly marked extent, of individual differences.

The apparatus used was an 800-cycle vacuum-tube oscillator. A condenser was used to act as a filter to purify the wave-form. The tone, which was pure apart from AC hum, was produced in head-phones. Since the apparatus was not calibrated, no measure in absolute acoustic energy, nor in db level, was available. Relative sound power, however, was taken to be proportional to the phone-current squared. The intensity changes were produced by depressing and releasing the key about three times per second. The/

The method used was that of limits, with complete ascent and descent. A check on the intensity-judgements was obtained by catch experiments, and by asking for judgements in the direction of change.

The complete results for twelve subjects are given in Table XXII, and the average results also in Fig. 41. The figures for Average

TABLE XXII.

Subject.	Relative Sound Power.						Mean
	.010	.040	.160	.360	.640	1.000	
I	.18	.11	.05	.07	.05	.06	.09
II	-	.23	.16	.19	.20	.21	-
III	.25	.11	.11	.11	.08	.06	.12
IV	.25	.11	.11	.11	.08	.09	.13
V	.60	.38	.17	.15	.11	.13	.26
VI	1.00	.38	.23	.19	.24	.23	.38
VII	.60	.38	.17	.19	.17	.11	.27
VIII	.25	.11	.05	.03	.05	.04	.09
IX	.25	.23	.11	.11	.08	.09	.15
X	.25:	.23	.17	.11	.08	.06	.15
XI	.25	.11	.11	.07	.08	.06	.15
XII	.25	.11	.11	.07	.05	.06	.11
Mean	.38	.21	.13	.12	.11	.10	.17
A.D.	.20	.10	.04	.04	.05	.05	.07
A.D./Mean.	.53	.48	.31	.33	.45	.50	.41

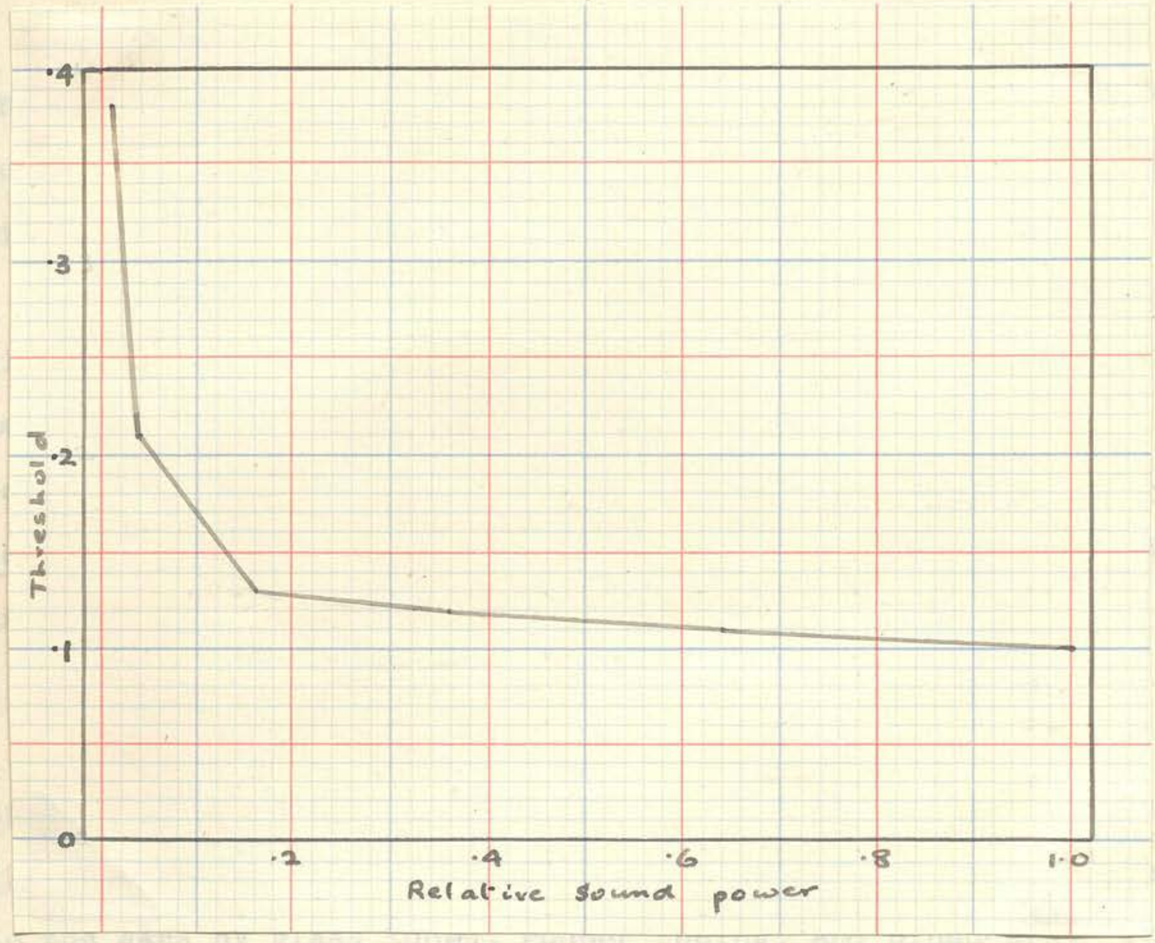


Fig 41.

Deviation/Mean show the extent of the individual differences. An indication of this can also be obtained from the column showing average threshold for each subject taking all six intensities together. The threshold was calculated from the formula $\frac{I_2^2 - I_1^2}{I_1^2}$ where I_1 and I_2 are the lower and higher intensities respectively.

The authors' conclusions are as follows: The value of the difference-threshold (or 'Weber-Fechner constant', as they call it) is in reality not a constant. From their own results it is shown to be a function of: (i) The absolute intensities of the tones compared. Values ranging from .10 to .38 were found for different intensities, taking the average of the observations of 12 subjects. The/

The value falls sharply at first, and then becomes much more nearly constant. (ii) The subject tested. Average values over 6 intensities ranged from .09 to .38. The mean average deviation was 41%.

From the results of other investigators they conclude that the difference threshold is a function of (iii) Frequency, especially among the lower frequencies - Knudsen (25). (iv) Duration of the tones: optimum about .3 sec. - Riesz (36). (v) Time-interval between tones: the shortest interval gives the finest discrimination - Knudsen (25).

A phenomenon which may be included in a study of 'Weber's law applied to the intensity of sound' was studied by Stewart and Hovda (242). Working with a tuning-fork tone of 256 cycles, conducted to the ears by glass tubes, rubber tubing, and binaurals, it was found that the ratio of the relative intensities in the binaurals produced an angular displacement θ of the 'fused' sound which would be represented by the formula

$$\theta = k \log_e \frac{I_R}{I_L}$$

where I_R and I_L were the intensities in the right and left ears respectively. In an extension of the first article, Stewart (243) added that the constant k varied for different individuals, and that the general 'intensity effect' did not exist for all individuals for all frequencies. Regions or bands of frequencies, apparently similar to 'tonal islands', existed, in which two sources of sound appeared to be present. The relation of this phenomenon to binaural localization in terms of phase-difference was also discussed by Stewart/

Stewart, but this point is not relevant to my present purpose. The chief interest of Stewart and Hovda's work is its extension to a type of experience involving the logarithm of the ratio of two stimuli, though this may be said to apply also to the logarithms of auditory intensities, which after all are measured in terms of ratio to some standard intensity.

A general conspectus of the results of the researches is given in Table XXIII, below. A few remarks by way of summary, however, are desirable here.

The general finding is that the Weber-Fechner law holds for sound intensity only over a middle range. The lower limit of this range is variously given, as 10^4 times the threshold of audibility by Fletcher (115), as 10^5 times the absolute threshold value by Riesz (36) and as not less than 10^6 times the minimum audible intensity by Banister (57). Free (123) gives both limits - 40 and 90 db intensity level. Many other writers, however, either ignore the 'upper deviations' from Weber's law, or mention them without specifying further. Difficulty in making observations near the threshold of feeling is probably responsible for defects in this respect. This partial verification of the law is interpreted in different ways. Extremes may be seen in statements on the one hand by Baron (58) who describes the differential threshold for sound intensity as <u>sensiblement constant</u> throughout the auditory range (though he adds that at the extremes "la sensibilite decroit notablement"), and on the other hand by Shaxby (227),: "it is more than doubtful whether this [the Weber-Fechner] law is valid

for/

for audition".

As regards the values quoted for the ratio $\Delta R/R$, I have already indicated that the early investigators obtained an average value of $\frac{1}{3}$, while later work gave a value of $\frac{1}{10}$. In some cases still lower values have been quoted, but Dashiell (105) gives $\frac{1}{5}$, with limits of $\frac{1}{3}$ and $\frac{1}{8}$, and Kellogg (22) an average of $\frac{1}{8}$.

Many of the authors quoted give summaries of previous work. The most comprehensive up to its own time (1920) was that of Marx (30), but this of necessity contains little reference to work on tones, since this has developed along workable lines only recently. At present it seems next to impossible to investigate tones and noises by really comparable methods; the nearest approach is seen in noise-analysis methods, but these have seldom proved absolutely reliable, and attempts to produce standard noise-sources (e.g. by Crawford (102)) has on the whole proved unsatisfactory.

TABLE XXIII.

Date	Authors	Sound source	Range	No. of subj.	Method	Thresh.	Dev.& var.	See also Tables	See also Figs.
1856	Renz & Wolf (38)	W	1	2	RW	.38	-	II	8
1860	Volkmann (7)	P	144		RW?	k	-		
	"	FA	wide		?	.33	-		
1879	Nörr (34)	FA	$3x10^{5}$	1	RW	k?	-	IV	9
1883	Tischer (44)	FA	800	5	L	.4	-		10,11
1885	Lorenz (27)	FA	c.100	1	L,RW	.3	u	V	12
1886	Starke (40-1)	FA(W)	c.10	2	L	.3	-		13
1888	Wien (45)	TF(t)	10^{11}	few?	L,RW	.12 .2	u	X,XI	18
1888	Merkel (31)	FA(W)	10^{4}		L	c. .3	-	VI	16
1893	Kämpfe (19)	P	wide	3	RW	k			
1900	Ament (2)	P	3	2	L	c. .3	1		17
1904	Hoefer (18)	FA(W)	small	many	RW	not k	irr.		
1905	Deenik (6)	TF(r)	c.200		L	.2 .3	?	XII	20,21
	"	OP(r)	f		L	.1-.2	irr.	XIII	21
1907	Keller (20)	FA(W)	small	9	MC,L	c. .1	-	XIV,XV	22
1922	Guernsey (13)	VO(t)	f	6	L	.3	1	XVII	24,25
1922	Knudsen (25)	VO(t)	wide	4	L	.1	1	XIX	26-28
1924	Halverson(15)	VO(t)	c.200	3	RW	.2	1		29
1928	Riesz (36)	VO(t)	10^{10}	12	L	.1-.3	1;reg.		30-33
1929	Kellogg (21)	AO(t)	3		?	.1?	?		
1929	" (22)	AO(t)	wide?	5	RW	c. .12	irr.?		
1930	Macdonald & Allen (28,1)	V	c.10		L	not k	reg.	XX	34-36
1930	Kenneth & Thouless (23)	VO(s)	wide	2	L	c. .1	1;reg.		37-39
1935	Gage (11)	VO(t)	c.250		L	not k	1;reg?	XXI	40
1935	Telford & Denk (43)	VO(t)	100	12	L	not k	1;reg.	XXII	41

NOTES TO TABLE XXIII.

SOUND SOURCE. I have given only a general indication of the apparatus employed in each investigation. The abbreviations used are as follows:

W	:	Watch.
FA	:	Fall apparatus; FA(W) : Wundt's fall phonometer.
OP	:	Organ pipe.
P	:	Sound pendulum.
TF	:	Tuning-forks.
V	:	Variator.
VO	:	Vacuum-tube or valve oscillator.
AO	:	Audio- or tuning-fork oscillator.
(r)	:	Sound canalized through rubber tubes.
(s)	:	Sound produced in loud speaker.
(t)	:	Sound produced in telephone receiver, head-phones etc.

RANGE. This represents the ratio of the extremes of intensity, and is given bnly approximately. When insufficient information is given for a numerical indication, I have used 'small' and 'wide' usually according to the interpretation put on these terms by the author's themselves. "f" denotes that frequency was the principal variable.

NUMBER OF SUBJECTS. In some cases the authors unfortunately give no indication on this point.

METHOD. The abbreviations are:

L : Method of Limits. In many cases, especially in the more recent work from the physical stand-point, free adaptations of the method of limits were used. In all cases, however, some sort of gradual approach to a limiting value is to be understood.

RW. Method of Right and Wrong cases, including cases quoted as employing the constant method, etc. No differentiation is made as to distribution of equality judgements, definition of threshold, etc.

M.C. Manifold Cases or Categories; see p.109.

THRESHOLD. The values quoted here are intended to give only the youngest indication of the average value over the range where Weber's law may be said to hold. When two values are quoted, they represent values for different frequencies, except in the case of Ament ; see p.94.

"k" represents a constant value not actually quoted in the original.

DEVIATIONS/

DEVIATIONS AND VARIATIONS. Marked deviations of the threshold from its mean value over the range studied are indicated as follows: l - lower; u - upper. Marked variations over the range: reg. - regular; irr. - irregular. Small deviations or variations are not noted.

The last two columns indicate tables and diagrams in the body of the review giving fuller numerical results.

The next section is devoted to a fairly full account of attempts to determine the relation between stimulus and sensations in a completely different way, namely, by 'noise-measurement' methods. I have given this precedence over certain further more theoretical considerations, since it represents an important and comparatively new development.

V. MEASUREMENT AND ESTIMATION OF LOUDNESS.

[Before reviewing the work done on noise-measurement and the estimation of loudness, I have thought it well to give, in abridged form, the Proposed Standards for Noise Measurement, as adopted in June 1933, by the American Standards Association Committee on Acoustical Measurements and Terminology (291), published also with comments by Fletcher (117).] It is based chiefly on the work by Fletcher and Munson (185, 10), q.v. The proposed standards are as follows, and are useful here inasmuch as they define the terminology used in the subsequent pages:

since elucidate much of

[1. The reference intensity for intensity level comparisons shall be 10^{-16} watts/sq.cm. [A formula is given for the corresponding r.m.s. pressure under given conditions of temperature and pressure .]

2. The intensity level of a sound is the number of db above the reference level.

3. and 4. The pressure level of a sound is given by $20 \log_{10} P/P_o$, where P_o is the reference pressure - 0.0002 bar. The unit of pressure level is the db.

5. A plane or spherical sound wave of single frequency 1000 cycles shall be used for loudness comparisons.

6. The loudness level of any sound shall be the intensity level of the equally loud reference tone at the position where the listener's head is to be placed.

7. In observing the loudness of the reference tone, the observer shall face the source (which should be small), and listen with both ears/

ears at a position such that the distance from the source to a line joining the two ears is one meter.

8. The loudness level of a pure tone propagated as a plane or spherical wave in air, and having a given frequency and intensity level, shall be defined by the set of curves given in Fig. 1 (Fig. 55 in this paper).

9. Until more accurate data are available the relation between loudness and loudness level shall be given by the curve shown in Fig. 2. (Fig. 42 below).

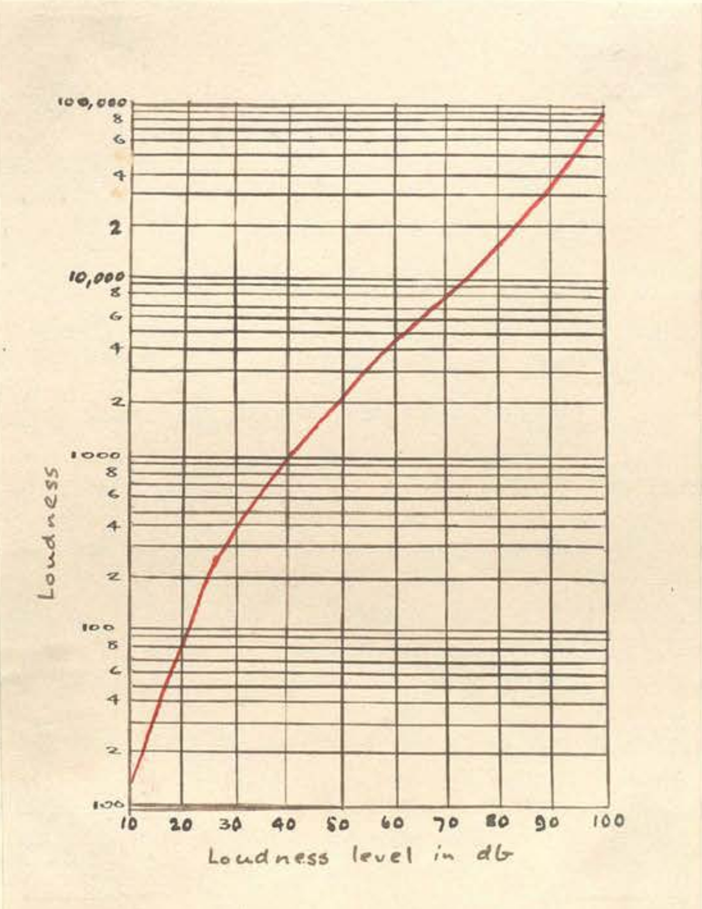


Fig. 42

The adoption of these standards makes the intensity level of the reference tone the same as its loudness level. This is true also for pure tones in a wide middle range of frequencies and intensities. [This report partly supersedes, and partly supple- (t) ments the definitions given by Fletcher (9) in the International Critical Tables. In particular, the International Critical Tables use two terms not used by the American Standards Association. (i) 'Phonic level' is used instead of intensity level, the 'zero' being taken as 1 dyne/cm . Personally I have found it most convenient to think of intensity level as the number of db. above the absolute threshold of the reference tone. (ii) Sensation level, a value not used at all by the American Standards Association, is defined by the excess of phonic level of a sound above that of P_0 , the threshold pressure value of that sound for the normal ear; this value depends on frequency. Sensation level, which I have used in my review, since it is widely used also by Fletcher in his "Speech and Hearing" (8) (a useful book which covers all the important work done up to 1929), may be conveniently thought of as the number of db of a sound above its own absolute threshold.

[As regards Fig. 42, it will be found that the Loudness values do not agree well with those of Churcher, King, and Davies (5), and the other suggested values quoted in Table XXVII at the end of my discussion of that article. A comparison made by Weston and Adams (276) is mentioned elsewhere. Further reference to this point is made elsewhere.

[It would seem therefore that if some definite functional relationship is to be found between sensation and stimulus, discrepancies of this sort will have to be eliminated.]

In/

In a paper on 'Sense of loudness' Sabine (221) described an experiment in which organ notes of seven different frequencies, at octave intervals, were balanced for loudness. Four small organs, separated so as to be out of range of each other's influence were used. The organs were so connected that any desired combination of pipes of the same pitch could be sounded. The sound energy of each pipe had been previously calculated in terms of the absolute threshold for a note of that pitch. The computations were made taking into account the shape of the room and the acoustic properties of the materials used in its construction.

The results, given in Table XXIV, are described as 'surprisingly concordant'. Ten observers performed the experiment. The 'musical characteristic' of each is noted, and it will be seen that there is no apparent effect of 'subconscious vocal effort', adjustment to the 'balance' of any given instrument, etc. The results for frequencies 64 and 4096 are not quite accurate, since all four pipes of the lowest pitch were not sufficiently loud, and the faintest of the highest pitch was too loud. On the basis of these results it is possible to construct a 'loudness contour' (Fig. 43) which may be compared with those given by Fletcher and Munson (10).

Sabine's/

TABLE XXIV.

	64	128	256	512	1024	2048	4096
1. Piano	7.0×10^4	1.7×10^6	4.4×10^6	8.0×10^6	15.0×10^6	9.6×10^5	4.5×10^3
2. Piano	7.0	1.7	4.4	11.2	9.2	12.0	5.2
3. Non-musical	7.0	1.7	3.6	8.9	6.3	9.6	4.5
4. Non-musical	7.0	1.7	3.7	7.7	14.5	14.4	5.6
5. Violin	7.0	1.7	3.5	11.7	13.9	8.0	3.5
6. Violin	7.0	1.7	4.0	11.4	15.5	15.2	5.2
7. 'Cello	7.0	1.7	4.2	12.0	13.4	9.6	5.1
8. Tenor	7.0	1.7	3.9	13.3	13.5	10.5	4.0
9. Soprano	7.0	1.7	4.7	12.9	17.0	9.6	5.4
10. Piano	7.0	1.7	3.5	13.2	14.5	8.0	4.9
Average.	7.0(+)	1.7×10^6	4.0×10^6	11.0×10^6	13.3×10^6	10.6×10^5	4.8 (-)
P.E.		0	.11	.42	.64	.47	

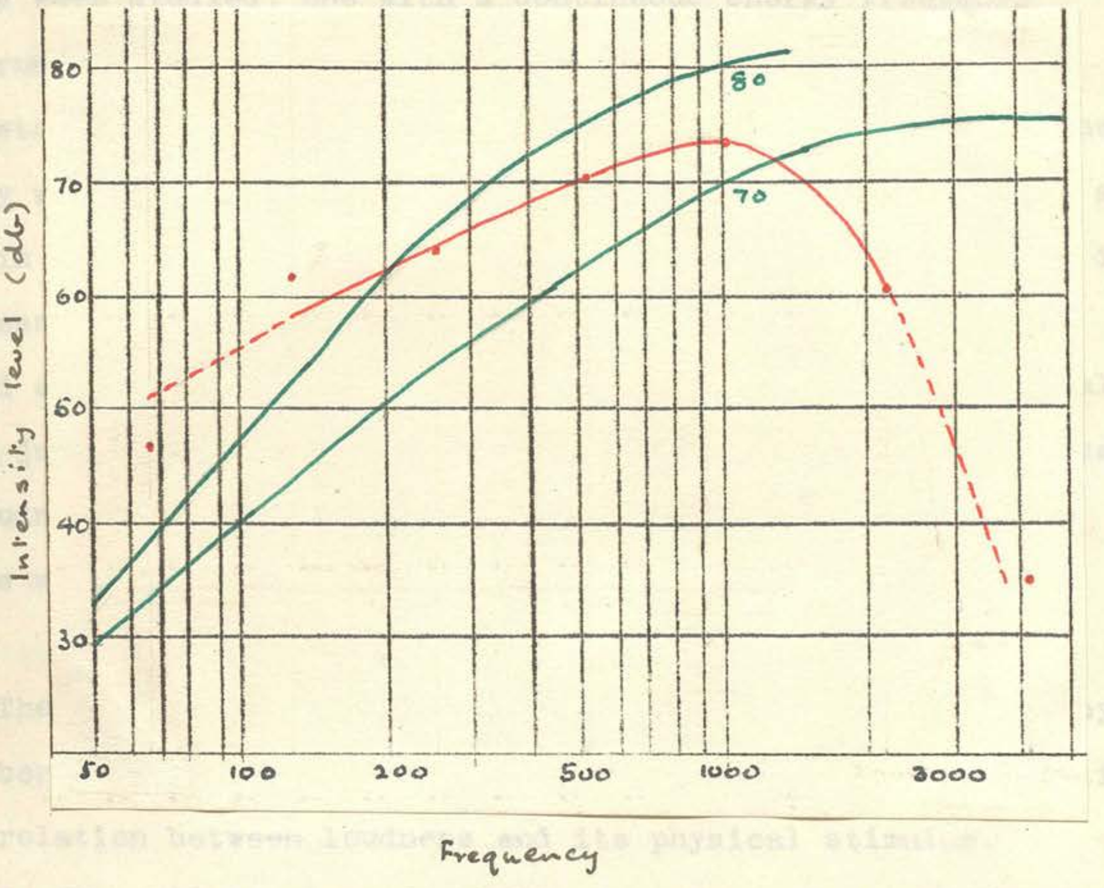


Fig. 43

Sabine's data are shown in Fig. 43 by a very crudely smoothed curve. For comparison I have given parts of Munson's 70 and 80 db contours (see Fig. 54). The curve for Sabine's data shows a fair degree of similarity, except for the sharp drop for the two upper frequencies.

In an early research on the estimation of overall loudness of a complex sound, Fletcher and Steinberg (118) showed that a total loudness could be obtained by summing a fractional power (root) of the weighted energy of each frequency region. Two complex sounds/

sounds were studied, one with a continuous energy frequency spectrum, corresponding to speech, the other a test tone with discrete frequency components. The general method was that the energy was removed from all frequencies either above or below a certain limit by means of filters, and the resulting decrease in loudness was measured by attenuating the original sound until judged equal in loudness to the filtered sound. The fractional power just mentioned was found to decrease to $\frac{1}{3}$ with increase in loudness to 100 'loudness units'. Calculated and observed values were found to be in good agreement.

The above investigation was later revised and amplified by Steinberg (42). The general aim in this case was the formulation of a relation between loudness and its physical stimulus.

Steinberg's development of his formula need not be given here, as it concerned chiefly with the treatment of the components of a complex sound, and with the conversion of sound-pressure values to sensation-level. It is shown that the total energy of a complex sound of k components is obtained by a summation of the type

$$\sum_{i=1}^{i=k} (W_i P_i)^{\frac{2}{r}}$$

where W_i is a weighting factor depending on frequency and sensation level, P_i the r.m.s. pressure of the i k component of the acoustic wave, and r a root factor depending only on sensation level.

The final form of the loudness equation is given as

$$L \text{ (Loudness)} = \frac{10}{3} \log_{10} \left[\sum_{i=1}^{i=k} (W_i P_i)^{\frac{2}{r}} \right]^{r^2}$$

this being a statement of the Weber-Fechner law in terms of physical units relating to sensation-level and pressure-spectrum. Curves are given showing loudness of several pure tones, using a form of the above equation adapted for pure tones.

Kingsbury (24) made a direct comparison of the loudness of eleven pure tones within a frequency range of 60 to 4000 cycles. The loudness of each tone was balanced with that of a 700 cycle reference tone by adjusting the voltage applied to a telephone receiver. The current was supplied by two oscillators. The intensities studied ranged up to 40 transmission units (or db) above the 700 cycle threshold, and the average results of 22 observers (11 male, 11 female) were plotted at each of these intensity levels to give contour lines of equal loudness. I have not given these, since they have since been superseded by the work of Riesz, Fletcher, and others. The general finding was that with a constant increase in amplitude-ratio the loudness of low frequency tones increases much more rapidly than that of higher tones. For frequencies above 700 cycles the rate is nearly uniform.

A series of curves is also given showing the relation between sensation level and loudness. The unit of loudness was the least perceptible increment of loudness of a 1000 cycle tone. The legitimacy of this procedure of course depends on the equality of such just noticeable increments, but Kingsbury states that this condition is fulfilled 'in the ordinary range of loudness', and refers to the work of Knudsen (25) from whose data it was possible to/

to plot a curve in which the 'Fechner ratio' was a function of the 1000 cycle sensation level. This, along with samples of Kingsbury's curves relating sensation-level and loudness, is given in Fig. 44.

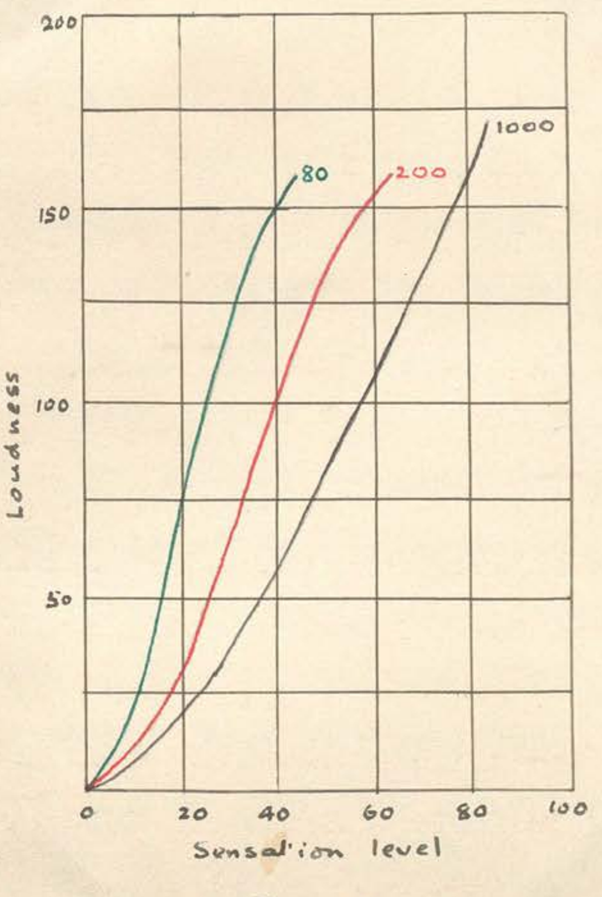


Fig. 44

In an appendix, a diagram is given showing the sensation levels of equally loud tones, as obtained from Kingsbury's experimental results, and from Steinberg's loudness equation. Kingsbury states that the computed values fit the experimental data rather well, although further work on the Fechner ratio would establish the results on a firmer footing.

In/

In an article entitled 'Loudness and telephone current' Richardson and Ross (39) discussed the possibility of numerical estimates of the intensity of auditory sensations.

An experimenter, who worked in a separate room, regulated the strength of the current in a current generator so as to produce variable intensities of a note of given frequency. Signals indicating that the stimuli were about to be given were made with a small electric lamp. A tone of pleasant loudness was first chosen as standard, and assigned the value 1.00. The observer judged the variables in terms of this (each variable was accompanied by the standard thus : SVSV), and wrote down his estimates. No information as to relative strengths was given until the observer had completed the entire experiment.

Of the 11 observers tested all were able to perform the task with some measure of success, although many found it difficult, or complained that they were only guessing. The methods of estimating varied a great deal with the observer. Introspection, however, except in the case of musical or otherwise skilled subjects seems to have yielded little definite information. When a plot of log j (current) against log l (loudness) was made it was found that for any one person the points lay near a smooth curve. For moderate and loud sounds it was found that the curve was nearly straight, indicating a relation $l = kj^n$. For fainter sounds the slope usually became greater, i.e. a sharper increase of loudness with current was found. As regards the index n in the above equation, it was found that it varied between .24 and 1.1 for different observers. The mean was about .5. The authors were unable/

unable to decide whether this indicated divergent wrong estimates of sensation, or sensations really different for different persons. A third possibility is that the numerical estimate is a function of the method of estimation, though of course sufficient data are not available in the work under discussion to test this hypothesis.

In no case was it found that the estimates conformed to the Weber-Fechner law $S = k \log R$ where S is 'intuitively' estimated sensation.

It was found that persons with poor frequency-sensitivity suffered from no disability in intensity-estimation. This result may be set against that of Norton (35) who seems to have found a good positive correlation between frequency discrimination and intensity discrimination, although the latter is only probably akin to intensity-estimation.

A paper published by Churcher and King (4) is interesting chiefly as a precursor to a later article (5). The authors here claim that the ear is the fundamental standard in noise measurement, although considerable subjective difficulty occurs, especially in respect of complex noise.

A curve given by Churcher and King which I have not seen elsewhere is reproduced and extended in Fig. 45; it shows the order of variation in differential sensibility that may be expected among persons having normal hearing. The differential threshold/

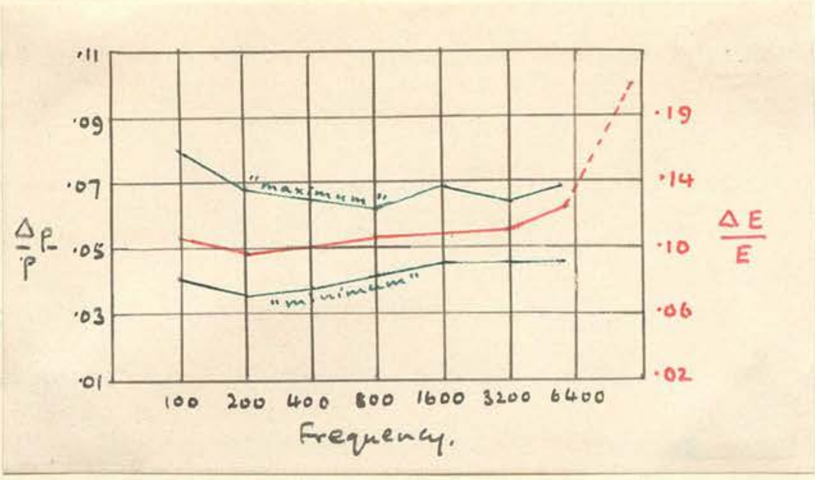


Fig. 45

threshold is given in units of pressure ($\frac{\Delta P}{P}$). Values for $\frac{\Delta E}{E}$ are shown in red on the right.

The greater part of the remainder of the paper is taken up with technical considerations. I refer elsewhere to some of the points raised. The authors conclude by proposing a loudness scale of 100 units, the loudness or 'nuisance' figure - for these are taken as synonymous - to be determined from the expression

$$100 \log_{10} (P/P_{min}) \,/\, \log_{10} (P_{max}/ P_{min})$$

where the p terms are expressed in dynes / cm^2, and P_{max} and P_{min} are determined from the data of Fig. 6.

In a discussion appended to the above paper A.H. Davis quotes Fletcher (8) to the effect that loudness calculations had not yet been successful. In particular, the formula proposed by Steinberg (42) was being gradually discarded. Davis also prefers measurement by reference to a standard tone, or by masking, to a 'loudness' or 'nuisance' scale such as the authors proposed.

Marvin/

Marvin (176) measured a number of noises by adjusting the loudness until judged equal to that of a 1000 cycle pure tone. The results were checked by means of an audio noise meter and analyser.

The experiment was undertaken in view of the fact that nearly all loudness work to date had been done with pure tones (this is perhaps an exaggeration), whereas pure tones are seldom met with in practical situations. The noises studied were those of a motor, a refrigerator, and a transformer. The levels were controlled by an operator in a separate room, in response to signals from the observer. The results in Table XXV are given in db above a reference sound pressure of 1 millibar, and represent the average of 8 observers, each of whom made two determinations at each level.

TABLE XXV.

1000 cycle tone level	17	22	27	36	44	59	74	88.5
Mean of meter readings	18.5	23	27.5	36.5	42.5	55	69	82
Meter error.	+ 1.5	+1	+0.5	+0.5	-1.5	-4	- 5	-6.5
P.E. of balance.	1.2	2.3	3.7	5.0	6.7	6.6	6.0	5.8

It is not quite plain from Marvin's article whether it was the meter or the aural method that was being put to the test. However, the results are fairly concordant. The author notes that a third weighting network to suit the higher levels would probably have improved these results.

Data/

Data showing the accuracy of the aural method of noise-measurement are given by Obata and Morita (194), who investigated noise on electric trams in Tokyo. Distribution curves of the errors show that these are greater when the noises vary rapidly in intensity or quality. Fatigue did not seem to increase the errors, which rather became smaller with the passage of time. New noise situations, however, increased the errors again.

Laird, Taylor, and Wille (26) approached the problem of translating decibel levels into the daily experience of the average person from a new angle. The method demands the selection of a sound which shall be a given fraction of a standard loudness. It thus approaches the "Method of double stimulus' mentioned by the early investigators, e.g., Merkel (32), and, less closely the numerical estimated method of Richardson and Ross.

The procedure was as follows: The buzz of a 3-A audiometer was sounded for 3 seconds, followed by 3 seconds silence, and another 3 seconds stimulation, but at a lower level. The observer judged whether this was half the previous loudness, or whether it required to be raised or lowered to give half the loudness of the original. The sounds were heard in one ear only. The stimuli were presented in haphazard order. The same procedure was carried out for reductions of one-fourth and three-fourths. All ten observers were normal in every way, and naive with respect to auditory judgements, since it was found that trained observers tended to identify the db levels.

The/

The estimates of half loudness were checked by 'doubling', i.e. by carrying out a similar procedure (with the lower levels) asking in this case for a loudness judged to be twice the standard level. A good check was obtained by this means, and the results, along with those of the one-fourth and three-fourth reductions are given in Fig. 46. A further check was made by using the relatively

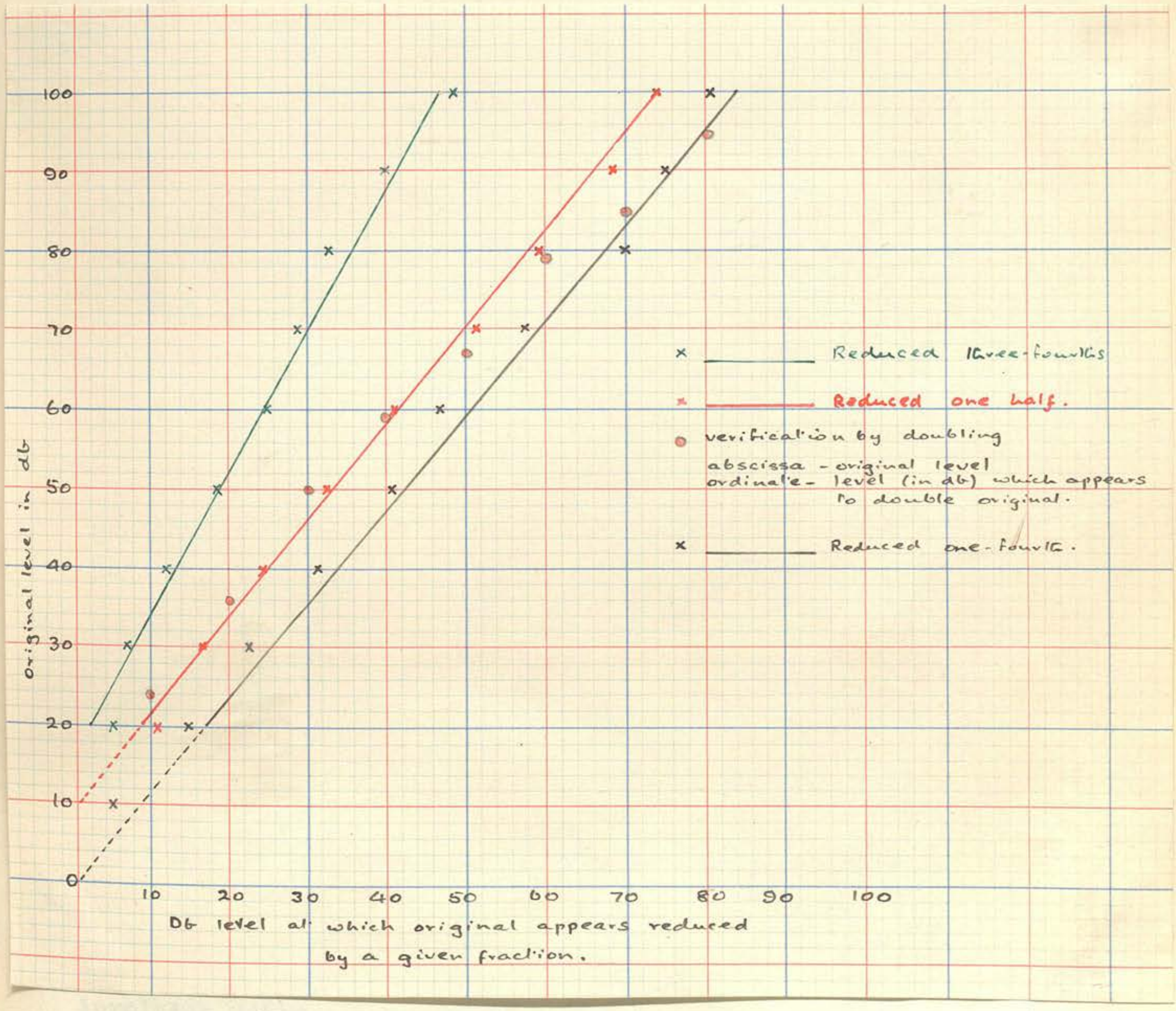


Fig. 46

pure tones of a 2-A audiometer, the frequencies being 64, 256, 1024, and 4096 cycles. In Fig. 47 I have shown the results of this part of the experiment. The figures attached to the curves indicate original level. In general, the results were practically

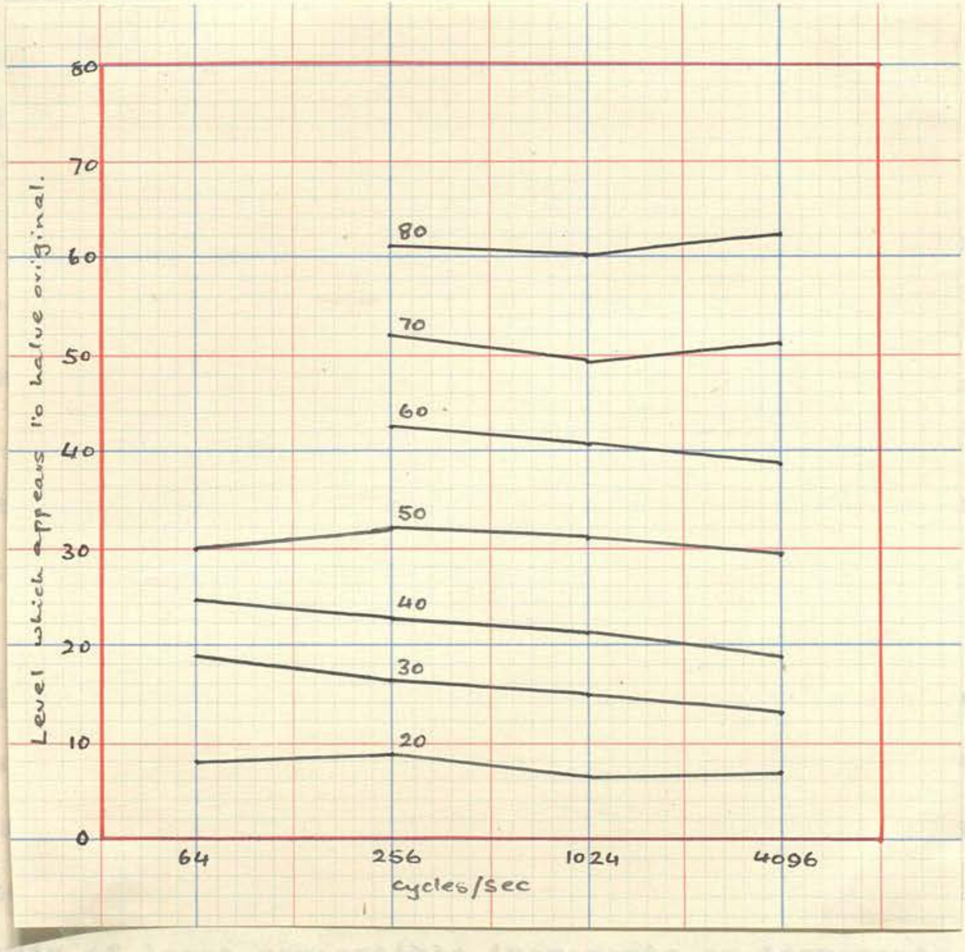


Fig. 47

identical with those of the buzzer tone, as were also those of a further series of determinations using a noise 'bathing the observer', this being closer to the conditions of factory work involving noise.

The/

The authors state what they call a 'tentative law': For a level of 80 db loudness appears to be reduced twice as much as the percentage reduction in actual db level, while at 30 db the percentage reduction in actual level and apparent reduction in loudness are about the same. Between these extremes there is an even gradation in 'illusory increment'.

Other noteworthy findings were:

(i) No marked individual differences were revealed. Indeed, a closer degree of agreement was found than in similar experiments on lifted weights - where there is more objective basis for comparison.

(ii) Correct identification of pairs of equal stimuli showed the non-occurrence of 'memory fading' - a phenomenon much discussed by the early experimenters.

More comprehensive than any of the researches so far discussed was that of Ham and Parkinson (16), who stressed the psychophysical or 'human' aspect of the noise problem. On the other hand, they departed from the general psychophysical methods which involve the study of least perceptible increments or decrements, since they considered it doubtful that such steps should be equal to one another in sensation. It was illogical, they held, to extend the scope of the Weber-Fechner law to cover all cases where the stimuli differed widely.

The general method employed was that a reference tone (R) was presented along with a comparison tone (C) of different db level, with a stimulus pattern RCRC. The subjects, all of whom were unpractised/

unpractised in making intensity judgements were asked to judge what percentage of the original (i.e. reference) loudness was represented by (or 'left in') the comparison loudness. Each individual stimulus was sounded for 5 to 10 seconds. This series of experiments was called Group A. Group B was carried out by presenting a reference loudness along with seven or eight comparison loudnesses, and the subject was required to select out of these that which appeared nearest to a given fraction: $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{1}{5}$. Group C was identical with Group B, except that a given multiple loudness (2, 3, or 5 times as great) had to be selected instead of a fractional loudness.

The stimuli used consisted of warble tones, single frequency tones, and room noise recordings, all being generated from special records by a Western Electric 201-A reproducer set. The range of frequencies covered was 250 to 2500 cycles, and the intensity levels varied from 34 to 84 db.

In Series A, each of the reference sounds was studied at 3 intensity levels, separated by about 15 db. For the 'high' and 'medium' levels 7 fixed db reductions had to be judged as percentages remaining of the original loudness; for the 'low' level, 6. The values given by the authors are the average of 18 determinations, and it is shown that the average deviation rises with intensity level. Curves for these experiments were drawn, plotting db reduction against the reciprocal of the remaining fraction of the original loudness. The curves obtained were of the general form of Fig. 48, whereas the Weber-Fechner law demands a straight line. The/

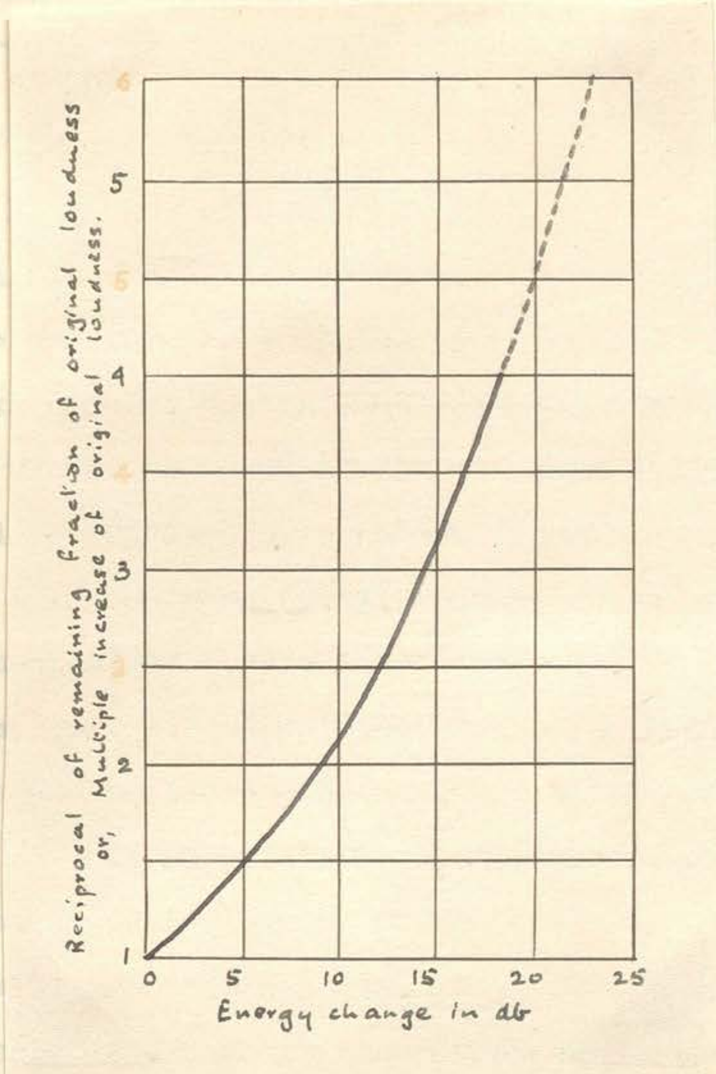


Fig. 48

The similarity of the curves, however, suggested a general relation of some other form, and the authors tried three types of equation:

$$y = a + bx^n$$

$$y = b(x + a)^n$$

$$y = a + be^{nx}$$

The third of these was found to give the best fit. Putting $a = 0$ and $b = 1$, it becomes

$$y = e^{nx}$$

The/

The authors compared this with a special form of the Weber-Fechner equation, which best suited their conditions:

$$m\,L = k \log (E^{m+1} / E)$$

where $m\,L$ is a multiple difference of loudness. The comparison gives $y = m\,L$ and $x = 10 \log (E^{m+1} / E)$

An examination of the curves showed that n might have different values for each of the standard sounds. Values of n were found by solving the equation $y = e^{nx}$ with values of y and x chosen from the experimental data within certain limits, and the theoretical curves thus obtained were shown to correspond within the limits of experimental error with the 'observed' curves for values of y between 1 and 5. These correspond to reductions as far as $\frac{1}{5}$ of the original, below which work was difficult, owing to room noise.

A similar procedure carried out with the results of Series B gave closer correspondence. In other words loudness judgements of this type were more accurate. This, incidentally, is an argument in favour of more orthodox psychophysical methods, as against numerical estimates. Tables and curves for Series C gave still closer correspondence.

Fairly wide variations in the value of n suggested that there was no consistent relation between it and initial loudness level. It was suggested that the different values might be due to some extent to background noise and other extraneous conditions. The closest general value, however, was $n = .076$.

Fig./

Fig 48, already referred to shows a theoretical curve for the equation

$$y = e^{.076\,x}$$

This, it may be shown, may be approximated to

$$m\,L = 1 + .76 \log \frac{E^{m+1}}{E}$$

which is a form of the Weber-Fechner equation. This, of course, is only an approximate statement of the experimental observations, and is only useful as the nearest approximation to the Weber-Fechner law, which was shown to be inapplicable to the entire range of data.

A further extension of the equation $y = e^{.076\,x}$ was made to give a relation between percentage reduction in loudness and a given/

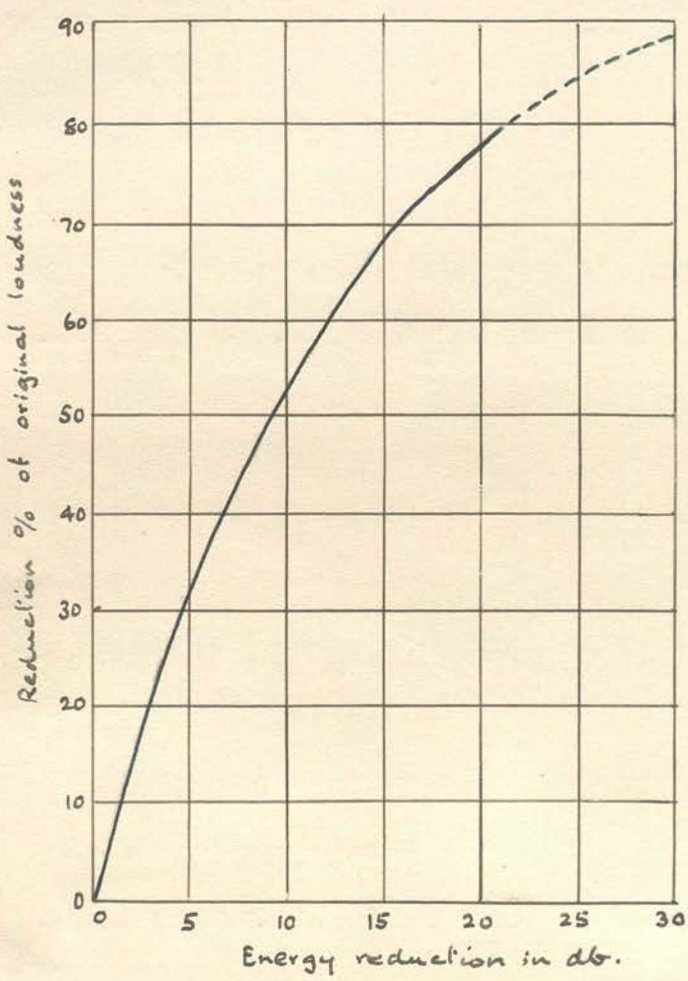


Fig. 49

given db decrease. It was shown that

$$y = \frac{E^{m+1}}{E}^{.331}$$

or, approximately, that loudness was proportional to the cube root of the db level. Fig. 49 gives this relationship.

The conclusions made by Ham and Parkinson are somewhat as follows:

All individuals having normal hearing (175 subjects were tested) have the mental or physiological equipment required to make judgements on the relative loudnesses of tones and noises at different levels. Each individual's judgements are consistent over a wide range, though they may differ from those of other observers.

A general law may be stated by the formula

$$m\,L = a + be^{nx}$$

i.e. the change of db energy is a logarithmic function of the change in loudness. Or, to put it the other way, change in loudness is an exponential function of change in energy, this being quite the reverse of the Weber's law relationship.

From the results it is possible to make a noise measurement scale. This is given in Fig. 50, which shows the relation between decibel level and multiple loudness units, which can be taken as a real measure of the magnitude of sensation. Two curves are given - one in db above the actual absolute threshold, the other in db above an arbitrary threshold of 1 millibar.

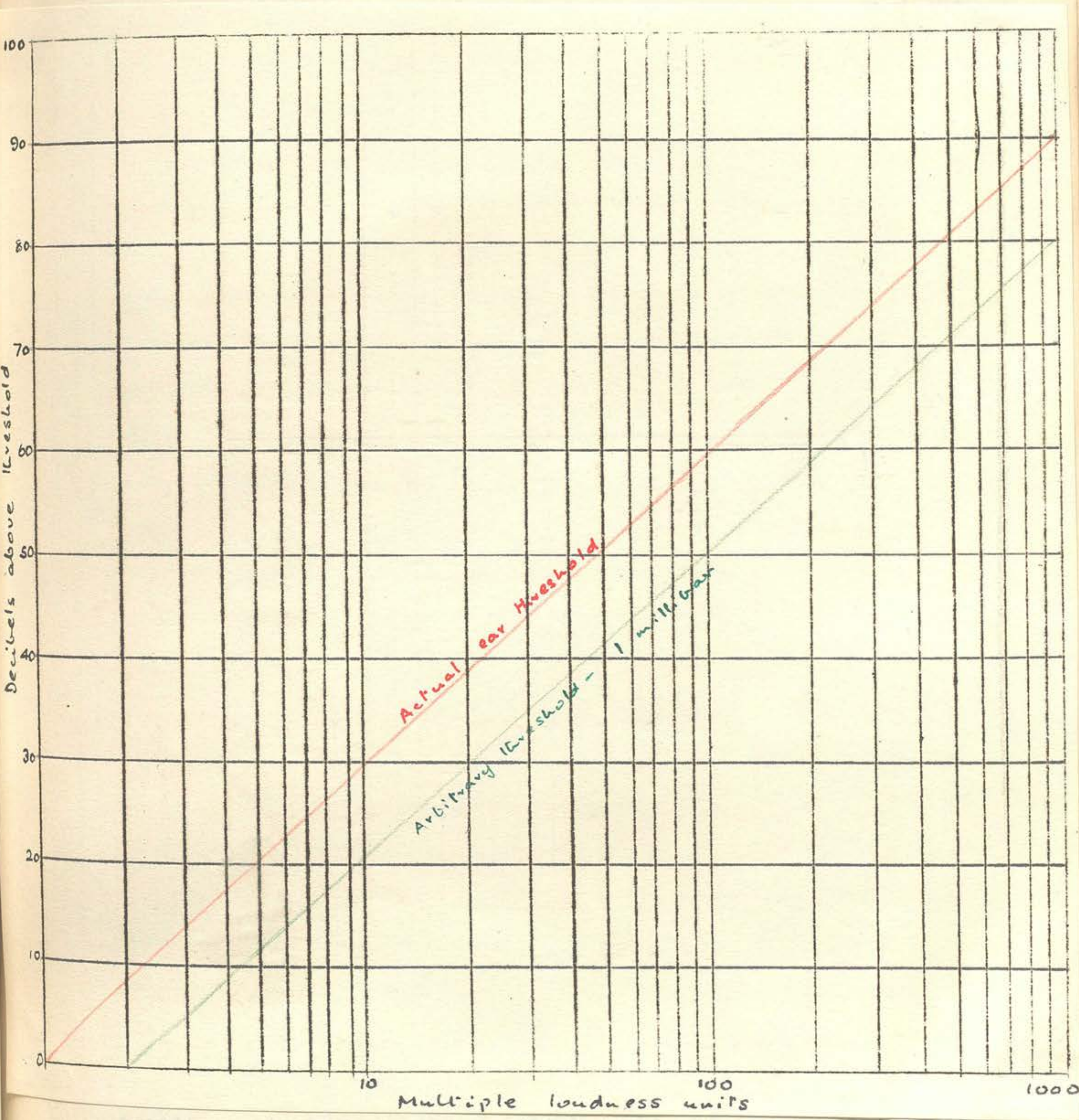

Decibels above threshold
Multiple loudness units
Actual ear threshold
Arbitrary threshold - 1 millibar
0
10
20
30
40
50
60
70
80
90
100
10
100
1000

Fig. 50

An experiment similar to the two preceding was carried out by Geiger and Firestone (12), chiefly in order to check and extend the previous results.

Headphones were used, so that fairly accurate knowledge of the intensities used might be obtained. A rather free procedure was used, - there being no restrictions as to time, number of stimuli, etc. The subject was given a switch which could be set at either of two positions: S or X. At position S , the sound was controlled by the operator; at position X , the subject controlled the loudness. The subject's absolute threshold having been previously determined, S was set at 30 db, and the subject was asked to adjust X so that it should be equal to S. The first estimate was by way of practice; the experiment proper consisted of setting X so as to equal $\frac{1}{2}$ S, $\frac{1}{4}$ S, $\frac{1}{10}$ S, $\frac{1}{100}$ S; 2 S, 4 S, 10 S, and 100 S. The same was done with sounds of 55 db and 80 db. Three different sounds were investigated: a 1000 cycle pure tone (which served also as reference tone) a 60-cycle pure tone, and a complex noise of more than 40 components, with an energy-frequency range similar to that of speech.

Tables are given showing the average and median values in intensity corresponding to the given fractional and multiple values of loudness. An examination of these yielded the following facts:

(i) The greater the loudness level, the greater is the intensity ratio between the original sound and the sound judged to be half (or other fraction) as loud. (ii) For low loudness levels, the change in intensity, measured in db , is greater when/

when the loudness is doubled than when it is halved. For high levels the opposite is true. (iii) A change in db corresponding to quarter loudness is always less than twice the corresponding change for half loudness. In other words, if the observer is asked to halve, and halve again, the result is less than that of quartering. (iv) A given fractional change in loudness does not correspond to the same fractional change in intensity for sounds of different frequency. This result agrees with that found by Kingsbury (24). This may be seen in Fig. 51, which shows smoothed

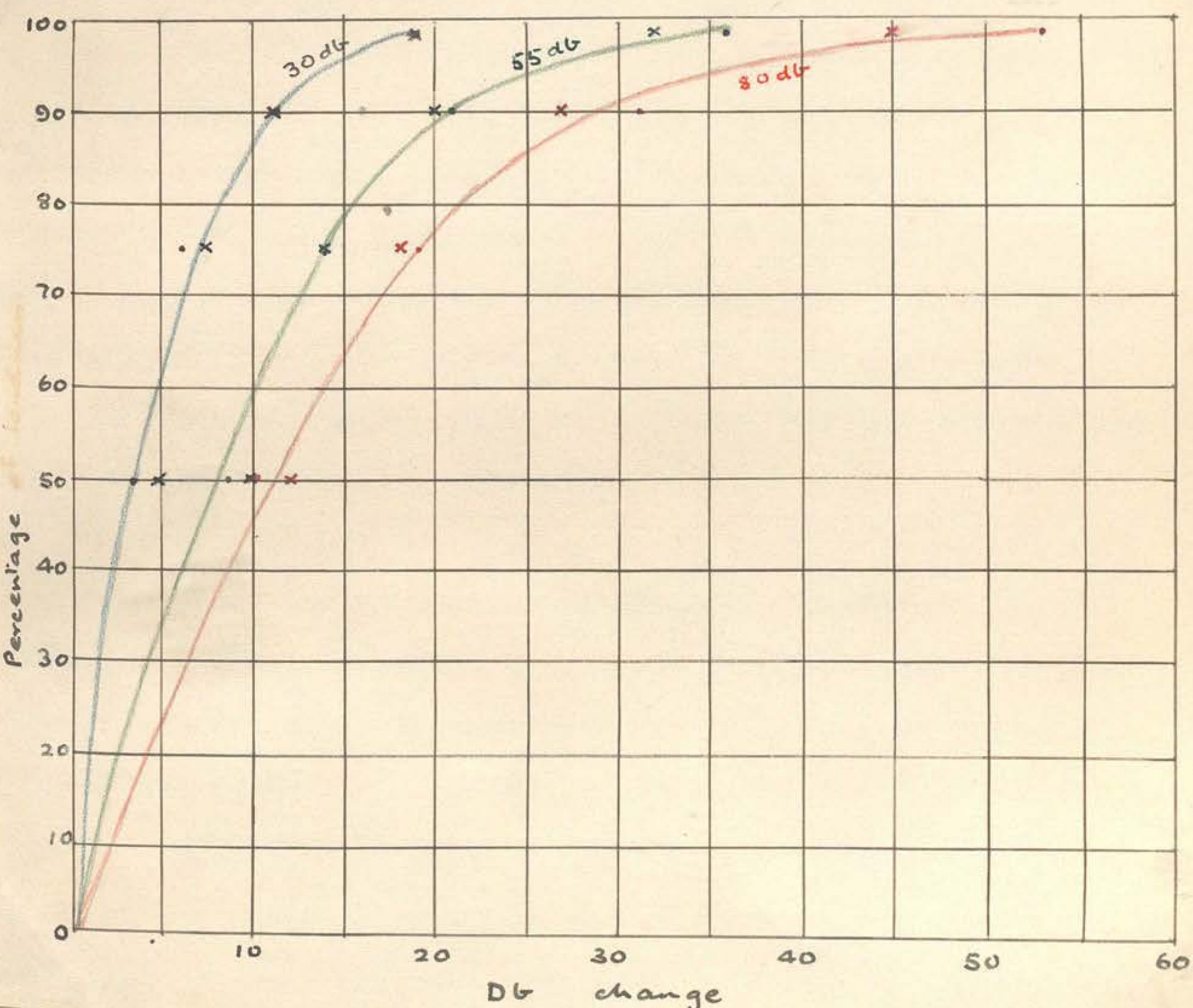


Fig. 51

curves obtained from the 1000-cycle data (dots), and the complex noise data (crosses).

The discrepancies between the present results and those of the other investigators, it is suggested, may perhaps be explained by the susceptibility of the observers to slight suggestion, as embodied in the initial position of X . The legitimacy of a method of experimentation such as those used in all three investigations is shown by the high degrees of consistency found, and by the fact that the observers all felt that such fractional and multiple estimates could be made. The authors conclude, therefore, that fractional loudnesses have a definite physical meaning. Possible methods of making such judgements were investigated by computing values of intensity changes based on just noticeable differences, and on imaginal distances from sources and numbers of sources. The results suggested that none of these was used. A good agreement, however, was obtained with data given by Fletcher (8) on the number of nerve impulses per second.

The general conclusion is that loudness judgements are made upon the basis of actual sensation.

Riesz (37) showed that tones of different frequencies, of which the intensities are the same number of minimum perceptible intensity-steps above the auditory threshold, do not necessarily sound equally loud.

The following alternative hypothesis is discussed. Two tones of different frequencies will sound equally loud when their intensities are such that the ratios of the number of distinguishable steps/

steps above the threshold to the number of distinguishable steps above the threshold for a reference loudness at the same frequency are the same for the two tones. This hypothesis was put to the test by using as reference the fifth of Munson's equal loudness contours (10). The results are shown in Fig. 52. The reference contour is shown in red, and the

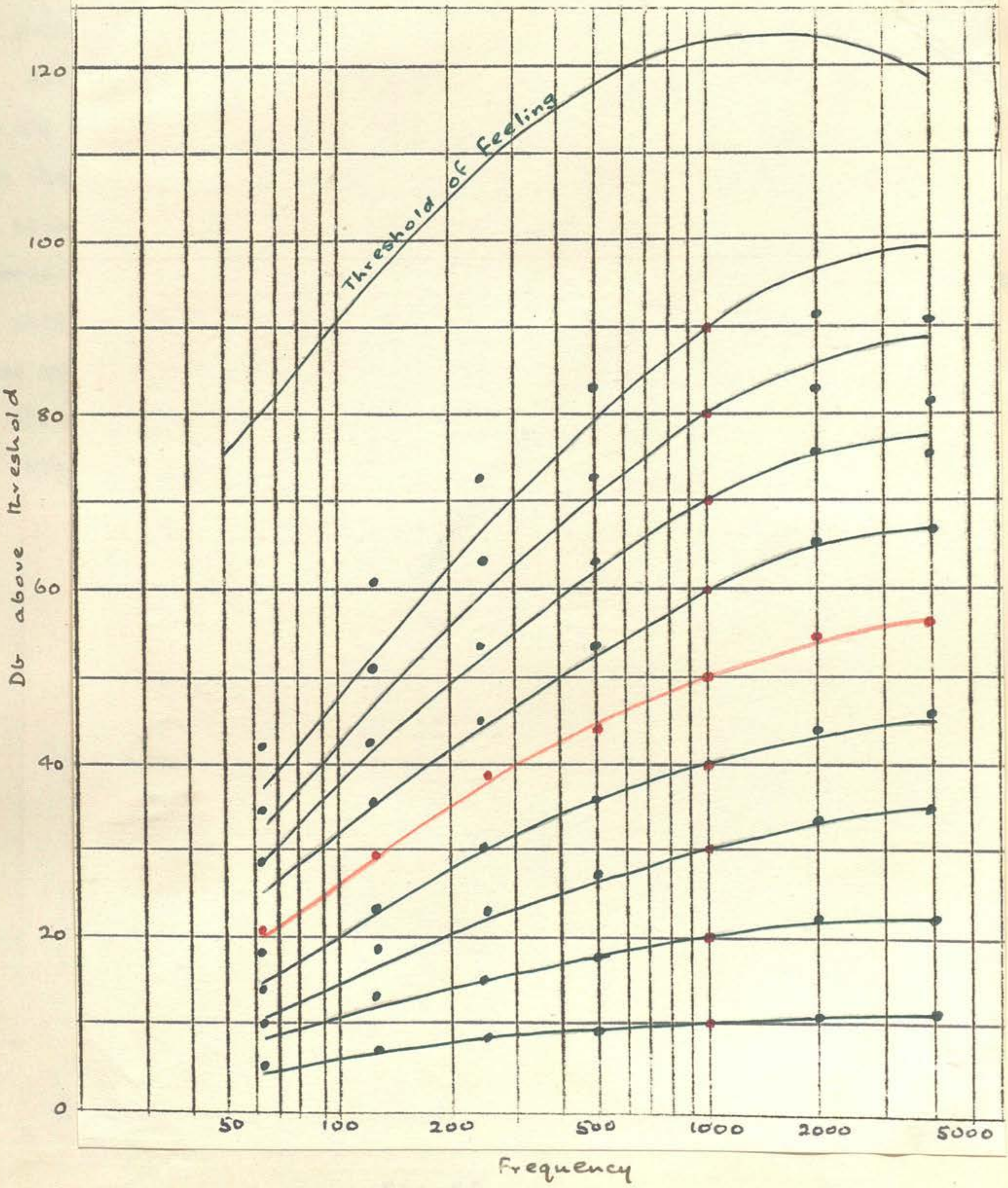


Fig. 52

numbers attached to the contours are the constant ratios mentioned above. Munson's other contours are indicated by dots. The theoretical contours based on Riesz's hypothesis are shown in green, and it is seen that they agree fairly well with those obtained by Munson's method, except at the two highest intensities. Riesz suggests that these discrepancies may perhaps be due to the influence of the threshold of feeling.

Riesz also discusses the recent work of Taylor, Laird, and Wille (26), Ham and Parkinson (16), and Geiger and Firestone (12). It is shown that these investigations gave results consistent each within each, but not with each other. Efforts of experimental conditions are evident, especially in the last-named, but the exact reasons for the discrepancies are still doubtful. Accordingly, as Riesz says, the significance of these and similar measurements must remain somewhat uncertain, until the discrepancies can be satisfactorily accounted for or eliminated.

Fig. 53 shows the extent of agreement between Laird, Taylor, and

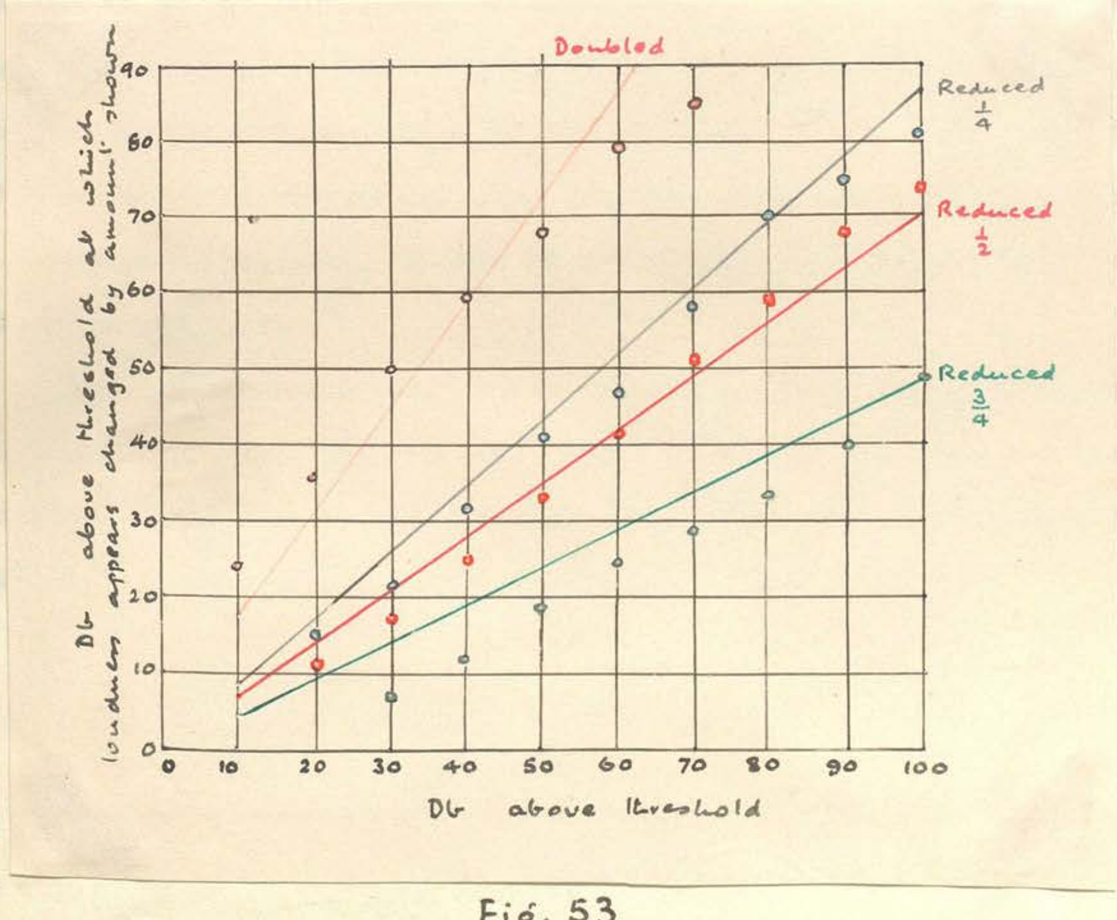


Fig. 53

Wille's average results on fractional loudness (indicated by dots) and the theoretical results on the basis of the hypothesis proposed by Riesz. The agreement is shown to be best for half loudness, perhaps because this was the judgement easiest to make. The results of the other two researches, on the other hand, do not give the same agreement, and therefore it cannot be assumed that the determination of a sound n times as loud as a given sound (whether n be fractional or otherwise) depends on that sound being n times as many just distinguishable steps above the threshold.

The most authoritative American work on the measurement of loudness is that reported by Fletcher and Munson (10). The experimental part of the work is described by Munson (185). The intensity levels at which pure tones of different frequency sound equally loud was determined by comparison with a 1000 cycle reference tone. The range covered was 62 - 16,000 cycles, at all intensities. Both ears of 11 observers were tested, the sound being given in a telephone receiver. Tones of short duration were used, in order to avoid fatigue.

The results are seen most plainly in the 'equal loudness contours' of Figs. 54 and 55. Fig. 54 shows smoothed curves of constant loudness (equal to that of a 1000 cycle reference tone at/

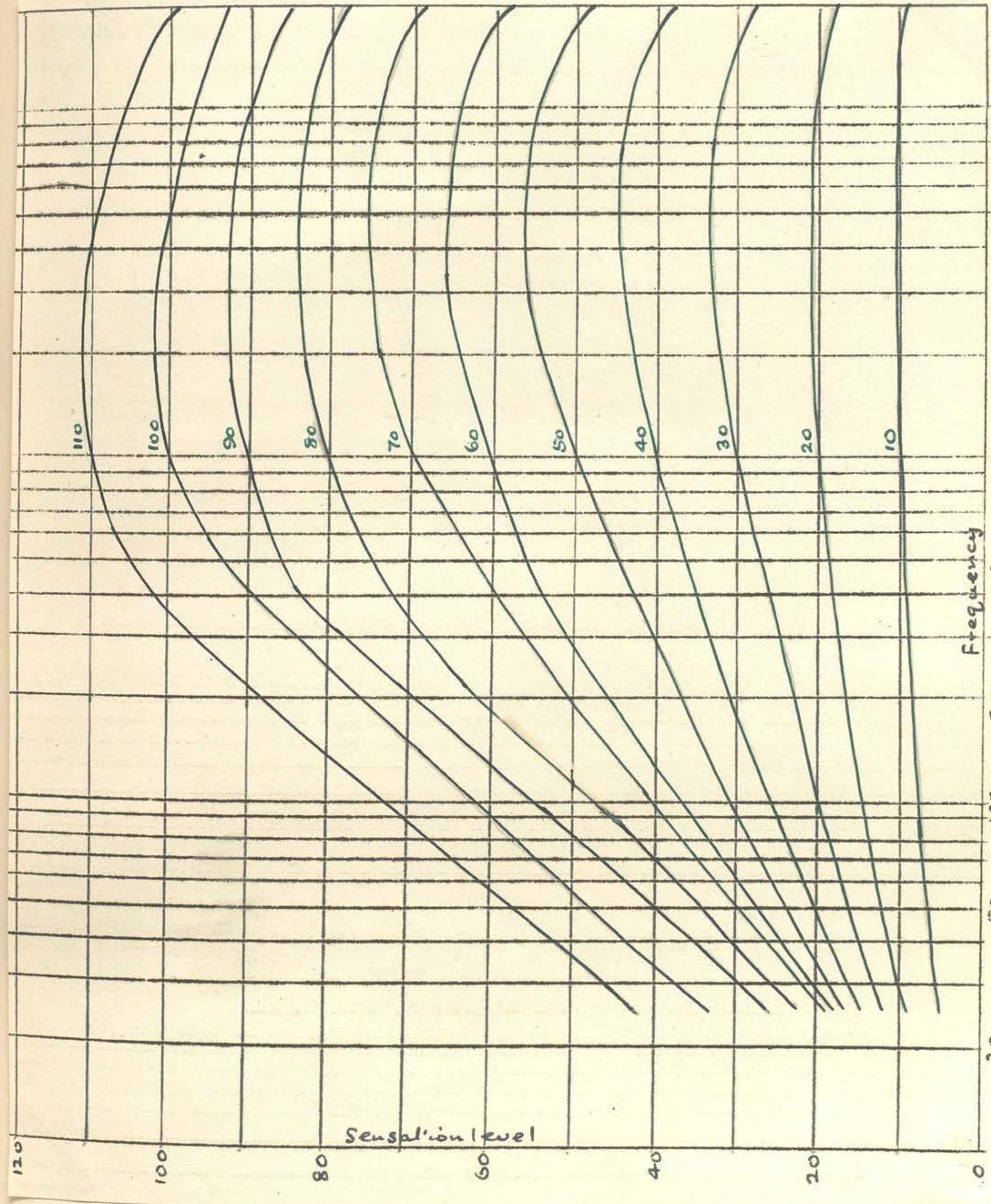

Frequency
10,000
5000
2000
1000
500
200
100
50
20
110
100
90
80
70
60
50
40
30
20
10
Sensation level
120
100
80
60
40
20
0

Fig. 54

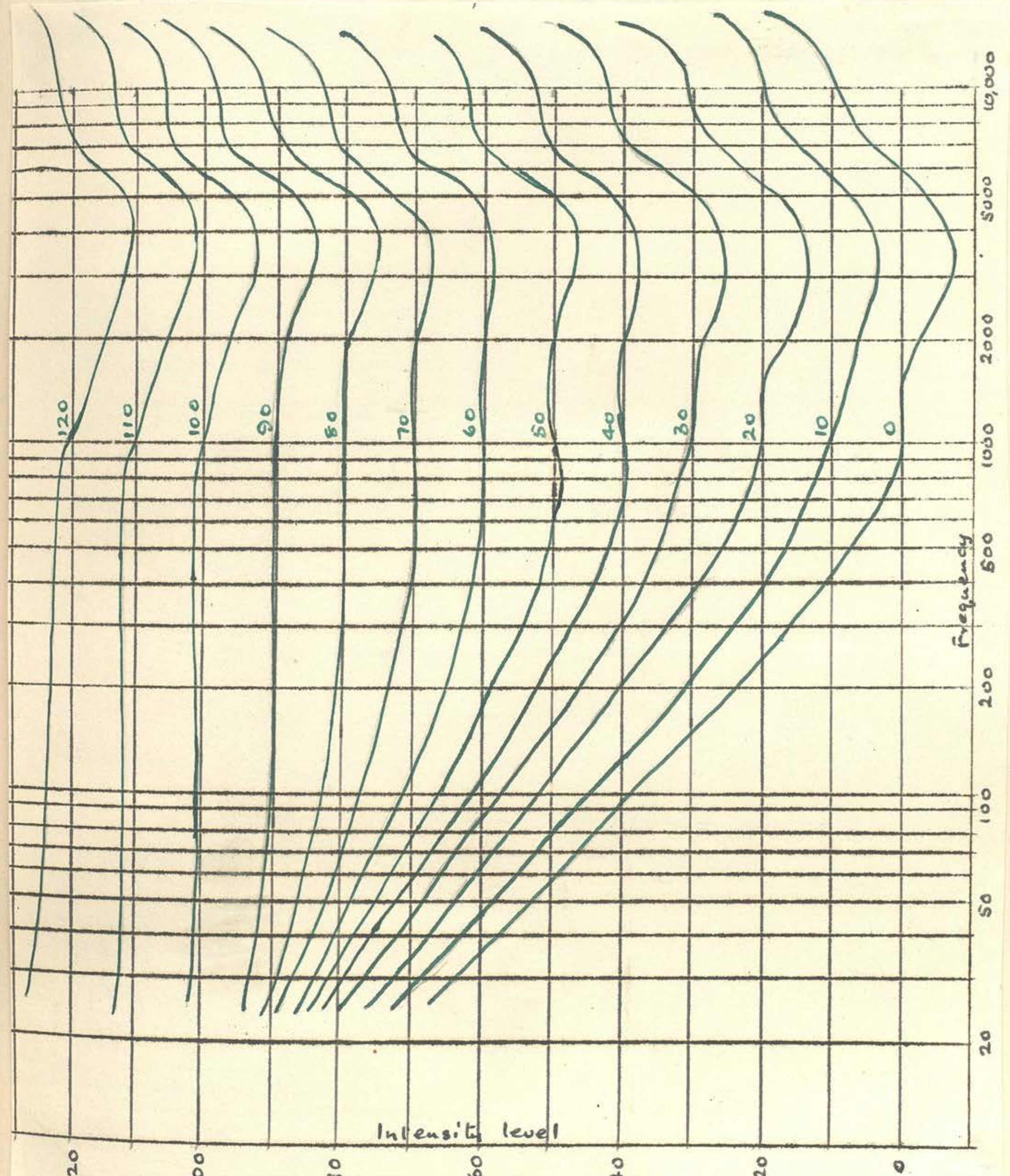

120
110
100
90
80
70
60
50
40
30
20
10
0
Intensity level
Frequency
10,000
5000
2000
1000
500
200
100
50
20

Fig. 55

at a given level) with sensation level (i.e. db above threshold value) plotted against frequency. Fig. 55 shows a similar set of equal loudness contours plotted with intensity levels as ordinates. In both cases, the figures attached to the curves indicate the intensity level (or sensation level) of the reference tone. The peculiar shape of the contours in Fig. 55, it was supposed, may be due to diffraction around the head of the observer as he sat facing the source. Negative values here represent sounds below the absolute threshold of hearing.

Fletcher and Munson devote the greater part of the paper to a formulation of the empirical theory for the calculation of the loudness level of a steady complex tone, this being a complicated function of the intensities and frequencies of the components. A formula is derived, which, however, is too complicated to be useful for general psychological purposes.

As I indicated earlier, the work of Fletcher and Munson, just described, largely formed the basis of the Proposed Standards adopted by the American Standards Association. A statement of the salient points, with comments, was given by Alger (50), who indicated an immediate programme for future research. The main point was the development of auxiliary standards for total-noise meters. Results to date suggested that a level of 60 db was the most suitable for general work, along with 30 db and 90 db as representative of high and low intensities. Alger further shows that sound-level depends on environment almost as much as on sound-energy leaving the source. To obtain 'nuisance' value, one must/

must go further, by means of empirical determinations. In all cases room noise should be at least 10 db below the noise being measured. This figure may be compared with that of 20 db given by Churcher and King (4). An amusing statement of the complexity of the problem of noise-measurement is made by Alger when he compares the task of measuring the noise emitted by a machine whilst running, to that of measuring the light emitted by a large luminous chameleon rapidly changing colour!

About four years after the publication of Churcher and King's paper (4), the same authors and Davies (5) revised and supplemented their results; in particular, they described the evolution of a new loudness scale specially designed for the treatment of engineering and other practical noise problems. Certain anomalies found in the use of their previous scale made a new scale desirable. The major cause of the anomalies was thought to be that Wegel's results, on which the previous scale was based, applied to measurements made with a telephone held close to the ear, whereas most practical problems involve 'free-space' listening.

I do not propose to enter into full details of the preliminary work towards the establishment of the scale, but shall limit myself to a brief outline, noting any points of outstanding interest.

A determination of the absolute threshold at various frequencies was made, using a valve oscillator and 48 observers (34 male and 14 female). The mode was taken as the average value. As regards individual differences, it was found that at 100 cycles the female group was on the average 2 - 3 db less sensitive than the/

the male group. The reverse was true at 800 cycles. A general reduction of sensitivity with age was found. On the basis of the results thus obtained, Kingsbury's data (24) on the sensation and intensity levels of equally loud notes of different frequency were checked and extended, but using free-space listening. The reference frequency was 800 cycles. Since only 6 subjects were used for this part of the experiment, Kingsbury's data were eventually adopted, with extensions to 100 db and 6400 cycles per second. The family of curves thus obtained is shown in Fig. 56.

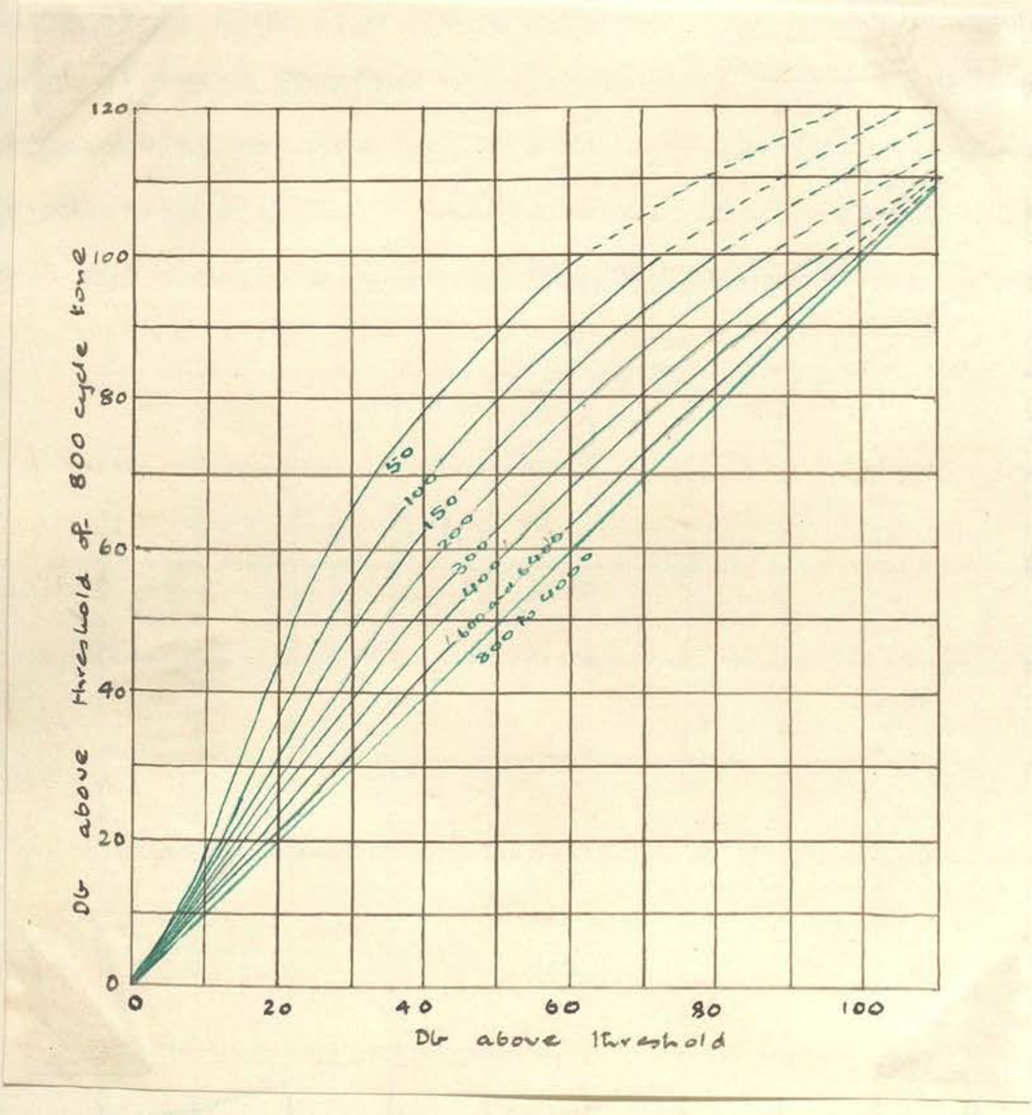


Fig. 56

These results, and experience generally, showed that the rate of increase in loudness with db above the threshold is comparatively small at low intensities, and much greater at high intensities. It was therefore thought desirable to establish a purely subjective loudness scale. This was done on the basis of the number of just perceptible increments contained in the intensity range from the absolute threshold to the level of the note under consideration. Such a scale had previously been made by Kingsbury(24), for a 1000-cycle tone using Knudsen's data (25), but it was shown that this had been done using cyclical (i.e. continuous) changes in intensity, which involved complications through the retention of impressions by the hearing system. The authors therefore worked with discrete changes in intensities, only incremental changes being used. The result is shown in Fig. 57 (blue curve). This relation

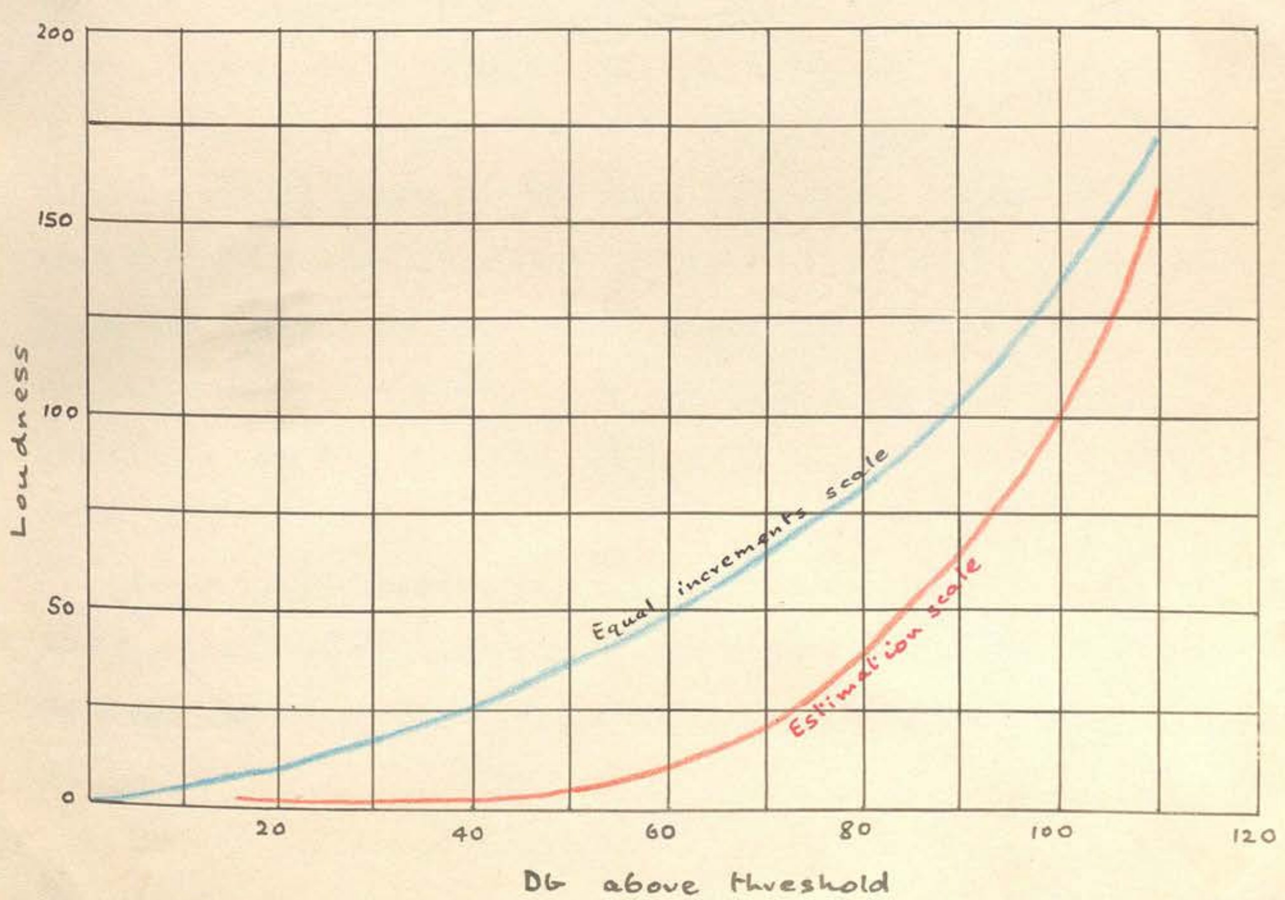


Fig. 57

was found to be more satisfactory than simply adopting the decibel scale as a loudness scale, but it still conflicted with introspectional evidence as regards loudness.

It was therefore decided to construct a multiple-loudness scale, using the method already tried by previous investigators (39, 26, 16.). Each of the subjects was furnished with a pair of headphones, which were fed via a two-way switch from either of two attenuators supplied with current from the same 800-cycle source. The subjects were asked to adjust the tone to half the original loudness. The procedure was progressively repeated six times, i.e. when the loudness corresponding to a db of 100 was also given an arbitrary numerical value of 100, the successive estimates would represent, respectively, loudnesses of 50, 25, 12.5, 6.25, 3.12 and 1.56. The judgements were subjectively vague, but fair day-to-day consistency was found, and another series of experiments in which quarter loudnesses were estimated (i.e. of 25, 6.25 and 1.56 units) also gave a fairly good check. Four skilled, and 30 unskilled subjects were used, and it was found that the two groups did about equally well. There was no tendency among the skilled subjects to identify db levels. The only remaining doubt was whether the quantity being measured was really sensation. It was, however, certainly what the ordinary person means by loudness.

Table XXVI shows the average results for half-loudness for all 34 subjects, and for quarter-loudness for the 30 unskilled subjects. Smoothed curves to fit these data are shown in Fig. 58, which may be/

TABLE XXVI.

Loudness Figure.	Ratio 2 : 1 Stimulus level db	S.D. db	Ratio 4 : 1 Stimulus level db	S.D. db
100	100		100	
50	84	3.7		
25	72	6.0	70	5.8
12.5	62	7.9		
6.25	52	9.5	54	7.3
3.12	43.5	10.5		
1.56	35.5	10.9	40	9.2

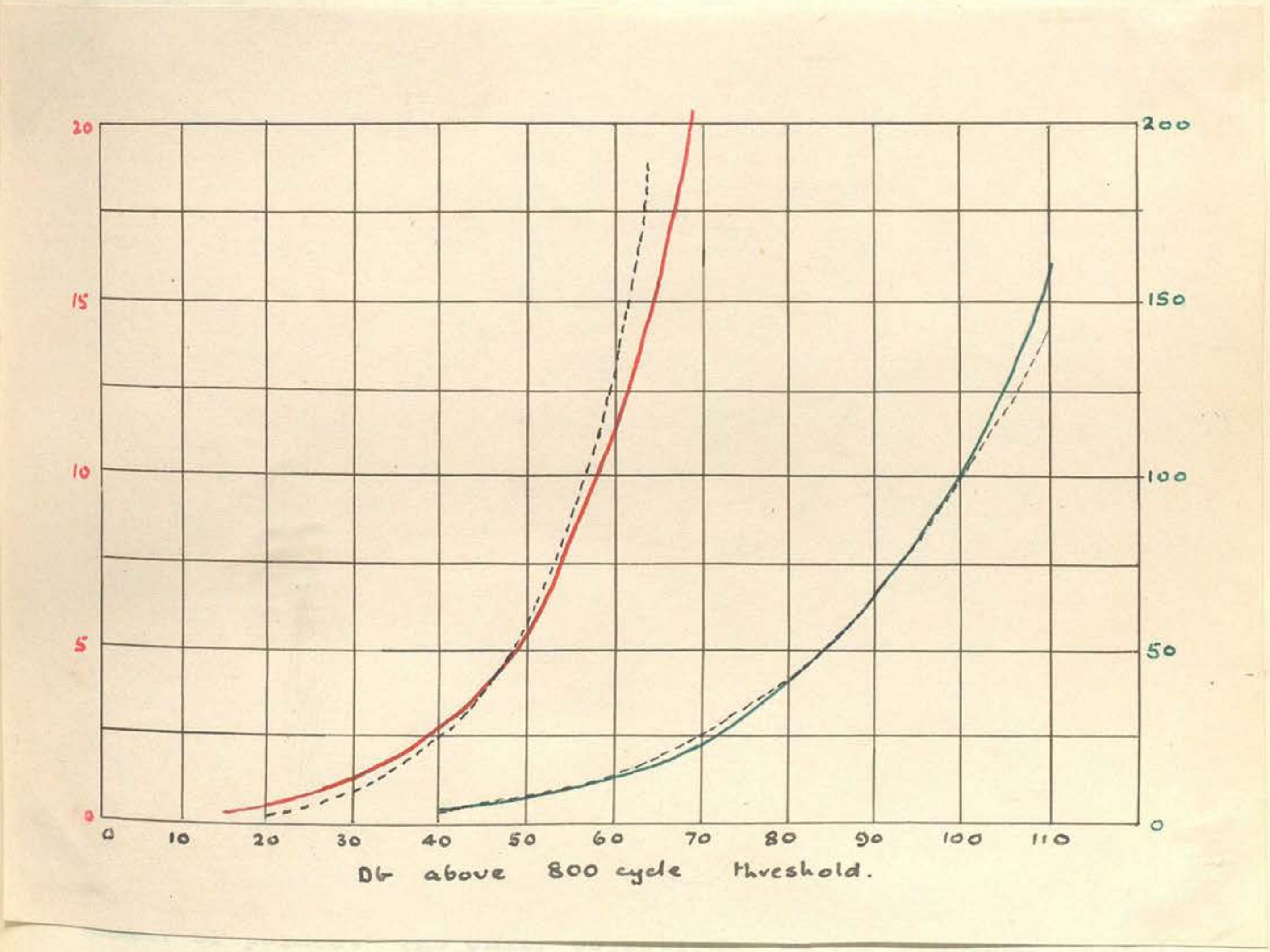


Fig. 58

be taken as the central feature of this investigation. The curve is shown in two parts, the red curve on the left being simply the lower part of the green curve on the right, extended, and magnified vertically ten times. The curve is also shown (in red) in Fig. 57 for comparison with the j.n.d. scale.

The above refers entirely to a reference tone of 800 cycles. The authors go on to describe the application of their loudness scale to the assessment of total noise, by an aural balance method. It is shown that even after analysis of the components of a complex noise the process of summation presents difficulties, due to a variety of making and interference effects, or perhaps to the presence of components outside the frequency range. It is shown that calculated loudness values based on analysis may yield discrepancies of anything up to 40% when compared with direct noise-meter readings. The use of an 'artificial ear' is further shown to be both cumbersome and inaccurate.

A comparison of different methods of listening was made, and one of these chosen. In essence it consists of the subject listening to the noise to be measured with one ear towards the source, while the other is covered by the telephone receiver, in which the reference tone is simultaneously sounded. The subject himself adjusts the oscillator voltage until the two sounds are subjectively equal. The results of balances obtained in this way was checked (cf. Kingsbury (24)) by equating two notes with a complex noise, and with each other.

An extensive discussion appended to the paper touches upon a number of points. The chief objections to the proposed loudness scale/

scale are in respect of alleged 'cramping' in the region critical for practical problems. With these we are not directly concerned. Of more immediate interest are a number of suggestions as to numerical relations between stimulus and loudness. D.A. Oliver showed that the scale under discussion is almost equivalent to $(db)^4 \times 10^{-6}$. Curves of this function are shown by dotted lines in Fig. 58. A.C. Hutchison stated that loudness (L) can be obtained from the relation

$$L = 3000\, W^{\frac{1}{4}} - 0.303,$$

(where W is the rate of energy flow in watts / cm^2). Reference is also made to Colebrook (100) who claimed that loudness varies roughly as the square root of the sound-pressure. M.G. Say proposed

$$\text{Loudness} = \text{antilog } (db/50)$$

as a simple relation between db measurements and their effective physiological value.

I append for comparison a table giving loudness values calculated according to the various suggested formulae, along with the results obtained from an exponential equation

$$L = 100\, e^{.05x - 5}$$

where x stands for sensation level in db. This has been chosen as giving the most comparable values to those of the other methods,* and/

* An alternative expression, $1.5e^{.05x}$, which gets rid of the -5 in the index of e, might have been preferred, but the present form was chosen since it gave a loudness figure of exactly 100 to correspond to 100 db.

and is included on account of the strong case for an exponential equation put forward by Ham and Parkinson (16).

TABLE XXVII.

Db	Churcher, King and Davies.	Oliver	Hutchinson.	Say	$100e^{.05x-5}$
110	160	146.4	179.7	158.5	164.9
100	100	100	101	100	100
84	50	49.8	45.3	47.9	44.9
69	20	22.7	16.6	24.0	21.3
59	10	12.1	9	15.1	12.9
49	5	5.8	5	9.6	7.8
30	1	0.8	1	4.0	3
0	0	0	0	1	0.7

The values given are those of a table in the present article showing loudness and db values of various engineering and other noises.

VI. OTHER APPROACHES TO THE STIMULUS-SENSATION PROBLEM.

We are now in a position to characterise Weber's law, for sound, at least, as not more than an approximation to the truth. Attempts to arrive at a closer approximation may take two main lines of approach. It is possible, on the one hand, to attempt a modification of the Weber-Fechner law, i.e., to suggest an alternative formula, which shall given more accurate numerical results, or, on the other hand, to interpret the problems of differential sensibility from an entirely different view-point. But it must be noted that here, as almost always in attempts at classification, there are cases which fall into neither category, or between the two, or partly in both.

In the first place it is necessary to consider what Brown and Thomson (88) describe as the one-time chief rival to the Weber-Fechner law, namely Plateau's 'quotient-hypothesis', which may be stated

$$S = c R^k,$$

from which it would follow that

$$\frac{\Delta S}{S} = k \frac{\Delta R}{R}$$

i.e., the j.n.d. of sensation is directly proportional to the j.n.d. of stimulus. The criterion for such a proportionality is that the estimated mean stimulus between two limiting values shall be the arithmetic mean of the two values. This is most conveniently investigated by the method of mean gradation, or 'bisection', as in the work of Merkel/

Merkel (32) and Angell (3). Elsewhere (178) Merkel states that only if Weber's law can be established by both the method of limits and that of mean gradation, can one assume the equality of j.n.d's and the consequent logarithmic relation between stimulus and sensation. But if one adopts the psychological interpretation of the Weber-Fechner law, i.e., that it is a law of apperceived sensation, it is possible to accept both the 'difference hypothesis' and the 'quotient hypothesis', on the understanding that the latter refers to excitation and the former to sensation. Yet Merkel (32) claims to show by experimental results that the quotient hypothesis holds for sensation in the sense of subjective experience, - i.e. by a demonstration of the arithmetic and not the geometric mean as the estimated mean of two limiting values. In another article (179) Merkel claims the possibility of immediate comparison of sensation in all varieties of psychophysical experiment, so that it would appear that real sensation (or excitation) and apperceived sensation are for him the same thing. However, Merkel's result has seldom been verified, and the quotient hypothesis must apparently be set aside (as Plateau himself later did), on the grounds that it serves no useful purpose, and, indeed, lacks experimental verification

Grotenfelt (134) stated that the only way to investigate the stimulus-sensation relationship was to postulate Weber's law and find out how far facts deviated from it. Other ways have since been explored, but one or two cases of Grotenfelt's method remain still to be noticed, including a suggestion of von Sterneck (238) that/

that the law be regarded as a postulate and external stimuli be defined in such a way that the law holds. Seashore (224), again, propounded the question: Does Weber's law depend on real or apparent stimulus? From a study of the size-weight illusion, he found that the difference threshold in weight-lifting was a constant fraction of the apparent weight of the standard stimulus, and concluded that in whatever sense Weber's law has any validity, it must be with respect to apparent stimulus. It would appear that this conclusion can apply only to such experimental methods as the constant method and estimation in absolute values, i.e. those in which 'back' or 'side' comparisons are most liable to be made. In any case, it is not plain how an investigation of this hypothesis could be carried out under stable conditions, since the degree of the size-weight or other illusion would no doubt undergo alteration as soon as other variables were introduced into the general judgement situation.

No direct mention has yet been made of Delboeuf's 'sense-distance' hypothesis. This was made in 1878 chiefly to overcome the difficulty of applying magnitude to sensation. A full exposition of Delboeuf's argument was given by Titchener (252), and has been upheld by various psychologists ever since. Quite Recently (in 1932) it received support from Wm. Brown (87) at the British Association symposium on the stimulus-sensation relationship. Following Delboeuf, Brown claimed that direct mental measurement was possible in terms of contrastes sensibles or 'sense-distances', so that the/

the Weber-Fechner law could be re-written:

$$\overline{SS_o} = k \log \frac{R}{R_o},$$

where $\overline{SS_o}$ is a sense-distance, and S_o any finite intensity of sensation taken as a conventional zero, but not necessarily liminal.

This appears to satisfy von Sterneck's contention (238) that measurement of sensation is restricted to those cases in which sensation produces ideas of spatial or temporal quality. But it seems to me that Delboeuf's hypothesis makes no significant advance on measurement of sensation in Fechner's sense, since an 'idea of spatial quality' is no more a real attribute of sensation, say of sound, than is numerical magnitude in terms of just noticeable differences. A variation of the sense-distance hypothesis is used by Schjelderup (297), who developed a very general formula for a 'sensation-number' (a sort of index of order), on the assumption that the original intensity I of a stimulus decreases progressively after the lapse of a given series of times t_1, t_2, t_3 to values i_1, i_2, i_3 and showed that the value of i depended on both I and t. Schjelderup, just as I have done, concluded that the basis for sensation measurement in the work of Fechner remained undisturbed by the replacement of sensation-intensity by sensation-number.

A number of formulae suggested as modifications of or alternatives to the Weber-Fechner formula are noted along with the work on sound on which they are based, described in Sections IV and V.

I/

I append here a number of other formulae, mainly for sensation in general, but also including some specifically representing intensity discrimination for sound. Results of definitely different 'approaches' to the stimulus-sensation relation are not included in this section, but treated separately elsewhere.

Henry (17), already quoted, stated that his observations were well represented 'en moyenne' by the equation

$$S = K(1 - e^{-\lambda i^m}),$$

where S represents 'les numeros d'ordre successifs de la sensation', and i the apertures of the diaphragm of his instrument. Numerical values of the constants are given as follows:

$K = 1000$, $m = .30953$, $\lambda = .0021816$.

Nutting (193), dealing mainly with vision, obtained an equation of a similar (i.e. exponential) form in an attempt to formulate a modification of 'Fechner's law' to cover all cases down to the threshold value where $\Delta R / R. = 1$. The equation was shown to be of the form

$$P = a + be^{-z}$$

where z is a function of log R. Nutting noted that at high intensities the value of $\Delta R/R$ tended to rise again, but claimed that no useful purpose was served in extending the function to cover these, since in this region sensation became painful, and probably the mechanism underwent alteration. Special forms of the equation could be developed to cover different sense-departments, and expressions found also to represent sensibility.

A/

A very full treatment of the Weber-Fechner law as a special case of a generalized relativity law is given by Pauli (204), and successively extended by Dingler and Pauli (205), and by Pauli and Wenzl (206). I deal with these articles fairly fully since they represent some of the most important implications of one of the interpretations of the Weber-Fechner law which I have discarded, namely the physiological.

Pauli (204) claim~~s~~ed in the first place that the Weber-Fechner law must be interpreted in this way, and not psychologically, on the basis of a logarithmic relation between stimulus and nerve excitation, the latter being equivalent to the actual occurrences in the sense-organ itself. An approximately logarithmic function appears so frequently in nearly all quantitative measurements in psychology that an extension of the Weber-Fechner law to a general relativity law seems justified. This generalized law might be stated thus: A subjective magnitude alters with the variable on which it depends in such a way that it, at first quickly, and then considerably more slowly, approaches a limiting value.

So much by way of preliminary. In the second paper of the series, Dingler and Pauli (205) come to the following conclusions (the first three are by Pauli, and the second three by Dingler):

(i) The extraordinary number of approximately logarithmic relationships appearing in plant and animal life, as well as in psychology, has for long been unappreciated. These cover stimulus-response relationships and also growth phenomena, and appear so frequently that all other types of numerical relationship are eclipsed (<u>zurücktreten</u>).

(ii)/

(ii) This circumstance is expressed in the generalized relativity law given in the last sentence of the discussion of the previous article, substituting 'biological' for 'subjective'. The approach to the empirical maximum described above assumes the general form of a logarithmic curve.

(iii) This relativity law has a special significance in the preservation of life: circumstances to be biologically avoided are reached comparatively slowly; those that are beneficial, comparatively quickly.

(iv) A consideration of the Weber-Fechner law shows that in its logarithmic form the law expresses the simplest possible functional relationship, namely, that the fractional alteration of the just perceptible difference of stimulus is itself proportional to the stimulus.

(v) As regards the causal interpretation of the law: either (a) it must be explained by a statistical combination of effects of separate physico-chemical processes, e.g. the law of mass action and osmotic-electrolytic effects, or (b) an explanation must be sought in the distribution and / or cessation of excitation. A fully developed theory on this basis was not in existence at the time of writing.

(vi) No definitive form of the above theory which will cover all facts has yet been put forward.

The third of the articles quoted, that by Pauli and Wenzl (206) contains an account of experimental work in several sense-departments, though not including sound, and of tests of various physiological/

physiological theories and formulae. A table is appended comparing the formulae of four investigators. In each case Pauli and Wenzl note the chemico-physical assumptions as regards the production or dissimilation of decomposable 'exciting' substances, and as regards the relation betweeen some function of this process and amount of nervous excitation. Two of the proposed equations are exponential (one being nearly identical with that of Henry (17)), and two logarithmic. The authors decide in favour of one of the latter. [I myself am in no position to express an opinion in this matter, and, indeed, am not concerned with the pros and cons on either side, since I hold that these formulae, dealing as they do with excitation, have little real bearing on my main problem.]

The attitude adopted by Pauli and his collaborators appears to have been predominant about the time of the above articles (c. 1920-24); cf. Rich's review (215) of Weber's law, in the <u>Psychological Bulletin</u>, 1924.

Bénézé (65) proposed a simple new formula

$$R = \tan^{-1} E$$

as an alternative to the Weber-Fechner law. This formula represents simply the connection between stimulus and response as shown in a curve relating the two, and may be taken as accurate throughout, instead of only for a middle range, since it can be applied to any small portion of the curve. The author claims that its only disadvantage lies in the fact that tan formulae are not in common use.

Guilford/

Guilford (14), on the basis of Woodworth's work (284), proposed to recast psychophysical laws in the general form of a power function

$$\Delta R = k R^{n}$$

n

where k and r are experimental constants.

Treating the sound intensity data of Kenneth and Thouless (23), Guilford found that the equation

$$\Delta R = .01276 R^{.489}$$

fitted, except for the first five values, all of which fell below the absolute threshold.

Passing now to the more definitely 'different' alternatives to the Weber-Fechner approach to the stimulus-sensation relationship, we find a number of widely different approaches. In the following review I deal with these, as far as possible, in groups, taking the respective groups in approximately chronological order of the first representatives of each group. Here again one comes upon the inevitable difficulty of finding a dividing line between the various groups, but this is of little importance in a case such as this.

A new law and a new interpretation of the whole situation was proposed by Fullerton and Cattell (124), whose argument was somewhat as follows: Owing to the complex physical, physiological, and psychological antecedents of perception (such as those discussed/

discussed in Sections II and III), the same stimulus is not always accompanied by the same sensation. These variations are to be interpreted as errors of observation, and experience has led to the conclusion that small errors of observation are more common than large ones, and that an error is as likely to be negative as positive. In other words, judgements or estimates of sensation (and in these one is really estimating the stimulus), are governed by the normal law of error. The theory of probability further assumes that an error of observation is the algebraic sum of a large number of small errors due to special causes. In particular, temporal and spatial relations, as well as intensity, affect errors of observation, as do individual differences and variations in observation for each observer.

On this hypothesis, Fullerton and Cattell found that they could not accept any of the three usual interpretations of Weber's law, since all experiments by the usual methods seemed to determine an error of observation under varying circumstances, rather than measure a quantity of sensation. The authors criticized each of the standard methods in detail, objecting in particular to the use of the category 'equal' in the methods of limits and Right and Wrong cases, since this rested solely on introspection, and in practice showed great variability. If the method of Right and Wrong cases is to be used, and 'equal' or 'doubtful' judgements allowed, these were to be divided among the 'Rights' and 'Wrongs'; and, in any case, 75% of the required type of judgement was to be taken as indicating the threshold value, and not 50%.

The logic of the argument up to this point is fairly sound, and/

and I am inclined to agree with Fullerton and Cattell's conclusion in their discussion of the psychophysical methods, since both the method of Limits and that of Right and Wrong cases depend entirely on the fact that different points of apparent equality emerge in different cases, or that a difference may sometimes be perceived and sometimes not. This is also true of methods of mean error or mean gradation, which are definitely and deliberately based on the assumption of variability or error in estimation or judgement of sensation. Some further remarks on this and cognate points are included in Section VIII.

Fullerton and Cattell's further argument, including the formulation of their 'law' is less important, but I give it here chiefly for the sake of completeness. Partly on theoretical grounds and partly from empirical results (cf. Cattell 94), the authors concluded that the 'error of observation' (corresponding to Fechner's just noticeable difference) was proportional to the square root of the stimulus. They accordingly proposed the following, which has been termed the 'Fullerton-Cattell law', as a substitute for Weber's law:

"The error of observation tends to increase as the square-root of the magnitude being observed, the increase being subject to variation, of which the amount and cause must be determined for each special case".

The remainder of Fullerton and Cattell's discussion need not detain us. Suggestions are given as to means of combining various psychophysical methods so as to give the most reliable results. The method of Right and Wrong cases is recommended as the most accurate, provided sufficient observations are made. The number required/

required varies with the degree of practice of the subject. Certain other points are also stressed, notably the additional uncertainty introduced by previous knowledge on the part of the subject. Cattell (94) explored an interesting side-line when he proposed time of perception as a measure of differences in intensity. This method, he claimed, offered also an excellent means of testing differences in sensibility. Thus, for example, it takes a person with normal vision about as long to discriminate between red and green as between blue and yellow, while it takes a red-green colour-blind longer. This argument is not altogether flawless, and it is difficult to see how the method could apply to sound, where simultaneous presentation is next to impossible, except by continued alternation. The time of judgement (rather than of perception) has also been suggested as a criterion of certainty and accuracy. Kellogg (157) experimented along these lines with visual intensities.

The upshot of the work of Fullerton and Cattell is that the Probable Error takes the place of the difference threshold as a measure of fineness of discrimination. In this way the question of a logarithmic relation between mental and physical processes is completely avoided, and the j.n.d. is interpreted as a most probable rather than an absolute value. Accordingly, as Linder (172) says, the problem of determining a threshold becomes the problem of estimating the central tendency of a frequency distribution. Whether or not it is possible to accept a normal distribution of thresholds or errors of observation is a matter that is still open to question. Lehmann (169) did not accept the Gaussian law as applicable to psychophysical determinations, since he believed that errors in these/

these had asymmetrical distributions. Usually, however, the hypothesis (i.e., the normality of distribution of psychophysical judgements in the method of constant stimuli) is assumed to hold good.

The matter is fully treated by Thurstone (248, 249, ~~250~~ 247), who draws up a 'law of comparative judgements' in terms of the separation or difference between two physical stimulus magnitudes, and the variability or dispersion of the processes which enable the observer to identify these two stimulus magnitudes. The formula applies equally well to comparisons other than psychophysical, and is similar to that for the Probable Error of a difference. Woodworth (284) had earlier proposed a similar formula, showing also that the Weber-Fechner and Fullerton-Cattell laws represented limiting cases of this formula. Thurstone concludes by proposing a restatement of Weber's law as follows: The stimulus increase which is correctly discriminated in 75% of attempts (when only two categories of judgement, such as 'higher' and 'lower' are allowed) is a constant function of the stimulus magnitude. A further discussion shows how Weber's law and Fechner's development are not necessarily interdependent; the main conclusion is that the psychophysical problem concerns the relation between a stimulus series and the discriminal processes with which the organism differentiates the stimuli.

A comparable interpretation of Weber's law as a law of sensitive error was made by Ronne (218), who showed that errors of observation are due to the fact that a constant in a biological relation is never absolutely constant, but ranges about a mean value. Accordingly, one/

one cannot do more than forecast a most probable value of, say, a difference threshold. Boring (73), on the other hand, claimed in 1920 that theoretically it is possible to determine accurately the result of a given set of conditions. Given all these conditions, we deal with certainties of cause and effect. Apparent inequality of cause and effect must therefore be explained on the basis of an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event. Carr (93) found such an explanation in a hypothesis of increasing resistance and consequent dissipation of energy into other channels. With this may be compared the 'duplicity hypothesis' proposed by Eliasberg(111), who postulated the increasing intervention of an antagonistic system, or an inhibiting mechanism, which was held to limit the effects of stimulation. At the same time Carr showed that some other explanation is required for the fact that in some cases minimal increments of stimulus do not produce any apparent sensory effect. This phenomenon, which may also be found outside the sensory domain, is to be explained in terms of non-equivalence of units of measurement when the cause-effect relation involves a transformation of energy. This problem had previously been studied by Pradines (211), who considered the interesting point in Weber's law not the consciousness of difference between different amounts of stimulus, but the fact that we are unconscious for so long of any change. Pradines put forward the hypothesis that consciousness does not passively register increments, but exerts some sort of activity towards them. The exact nature of this activity might be investigated through/

through a study of the limits and exceptions to the law.

A somewhat similar approach was made by Solomons (235), who based his theory on the observation that sudden changes of pressure were directly perceived as such. The act of comparison seemed to be minimized or entirely lacking, but objectively constant pressure seemed to undergo fluctuations, as of attention. The explanation of all threshold phenomena was therefore to be sought in the well-known fact of the variability of brain activity under identical stimulation. Two stimuli must differ by more than the range of this variability for their difference to be perceived. This links up with Thurstone's work on the Probable Error of a difference as providing the basis for a law of comparative judgement. The probability of a given difference between stimuli being perceived is the probability of the difference between two values of the combined effect of varying bodily and mental conditions being less than the difference between the stimuli. One hundred per cent of correct judgements will be obtained only when the difference between the stimuli is more than twice the greatest variation in the combined effect of the variable conditions. This last links up in its turn with Werner's treatment (275) of auditory sensation on the theory of antagonistic complements in sensation.

p.

[In direct contrast to] methods of investigation based on a statistical classification or 'census' of introspective judgements And are those based on attempts to measure actual responses in the nervous system. All These, latter of course, either assume a physiological significance/

significance of the Weber-Fechner law, or seek to narrow down the range of possibilities by showing that a logarithmic relation definitely does not exist between physical stimulus and nerve excitation. Reference has already been made to work in this field; the present discussion is intended only to fill in a little detail, and to throw some light on the principal theories of hearing. primarily I am concerned with this work only in so far as the investigators assume or recognize its possible bearing on what Adrian (49) calls 'change in the content of mind'. Adrian holds that some sort of relation exists between nervous impulses and sensation, but that the relation is largely speculative, since ~~no~~ investigation of impulse discharges from human sense organs is possible, practically im- although one can argue to some extent from those of the cat and the frog, since the structures of their sense-organs and nerve fibres are similar to those of the human being. How far this argument by analogy is possible or legitimate is an open question. In any case I am not concerned with, and therefore I omit to consider work such as that of Zeliony (288) and of Dworkin who found ~~a~~ differential threshold for auditory intensity ~~in terms of the~~ techniques salivary reflex in dogs, or that of Crawford and Brundage (103) who described recent methods of testing the auditory capacities of animals.

Reference may be made to Adrian's 'The Basis of Sensation' (49) for an account of pioneer work, mostly done in the present century, of the general type now under discussion.

The underlying principle is that activity in a nerve is invariably accompanied/

accompanied by an electrical change. This had for long been recognized, but recent developments in valve amplification have greatly facilitated the detection of the smallest electrical changes with comparatively insensitive instruments. A second fundamental principle is that since nothing in the nature of continuous nerve activity is possible, an impulse does not vary with the strength of the stimulus. This is the well-known 'all-or-none' theory of nerve action which runs directly counter to any theory (cf., e.g., Gellé (129)) which relates the intensity, say, of sound, to the degree or depth of excitation of the auditory nerve. Adrian shows that the nature of a nerve impulse at any point depends only on the local condition of the fibre at that point. The 'message' signalled by a receptor varies only with respect to the total number of impulses and their frequency, and failure to give graded discharges to graded stimuli is due to the very rapid adaptation of a nerve fibre to stimulation.

Adrian may be said to have demonstrated conclusively for certain senses that intensity of stimulus (and possibly of the corresponding sensory attribute) is correlated with frequency of nerve response. How far this is true for audition has not yet been conclusively shown. Wever and Bray (278), by a direct experimental procedure in which electrodes were placed on the exposed auditory nerve of a cat, established a correspondence between frequency of sound and frequency of response. The same authors suggested (279) that each fibre responds at a frequency below that of the sound waves, but in strict synchronization with them.

When/

When the same tone is sounded at a higher intensity, the fibres respond at a higher rate, thus giving a greater total number of impulses.

This hypothesis was developed by Wever and Bray (279) into the 'volley theory', and fully discussed by Boring (79), who shows that the intensity of the stimulus produces corresponding variations in the amplitude of action current in the nerve as a whole, although every single fibre is responding in accordance with the all-or-none law. In this way the volley theory synthesizes the multiple fibre theory of intensity, according to which intensity depends on the number of fibres excited, and Adrian's frequency theory. The main drawback is that the volley theory (in which, in any case, intensity theory is in the nature of a side-line to explanation of frequency or pitch sensation) is not applicable to other important sense-departments, notably vision.

An interesting hypothesis advanced by Hoagland (146) on the basis chiefly of visual sensation, might also conceivably apply to hearing. The theory is in brief, that increasing intensity of stimulus activates more and more receptors as their absolute thresholds of excitation are reached. This theory has been developed by Houston (147, 148), in conjunction with his observations on the course of the difference threshold for visual intensity. Houston holds, in common with many other writers, that the reciprocal of the Weber-Fechner ratio, i.e. $\frac{I}{\Delta I}$ furnishes a more valuable measure of sensitivity, since the numerical value rises with increased sensitivity, and vice versa. If this is plotted against a logarithmic/

logarithmic scale of intensities, it is shown that the results fit a Gaussian or normal probability curve, truncated at the end representing higher intensities, at which the value of the Weber-Fechner ratio is normally found to rise. These phenomena are to be explained as follows:

One must suppose a population of percipient elements each connected with a fibre of the sensory nerve, and having different thresholds of excitation. A normal distribution with respect to these thresholds must further be assumed. A weak stimulus excites only a few (i.e., the most sensitive) of these receptors, and as the stimulus increases, the number of active elements also increases at a rate corresponding to the 'rise' of the first half of the probability curve. The 'peak' of the curve is reached when the group of receptors having the most frequently occurring degree of sensitivity has been brought into action. The subsequent decline in the curve is due to the fact that now a progressively smaller number of receptors corresponding to each degree of sensitivity is available. That the curve does not reach its previous lowest level is due to the fact that the more sensitive receptors are still active. Presumably the disappearance of normal sensation when the threshold of feeling is reached is to be explained by the complete activation of <u>all</u> the available receptors.

Sensation, according to this view, is given by the integral of the probability curve. Since this cannot be integrated exactly, it is impossible to plot sensation against stimulus accurately, but tables of the probability integral are of course available/

available. Houston (148) gives a curve of $\frac{S}{2\pi}$ (i.e., a quantitiy proportional to S) against log I , and shows that the straight line curve of $S = k \log I$ coincides with it over a certain middle portion, which can be taken as the range over which the Weber-Fechner law is approximately valid.

This hypothesis is admittedly largely conjectural, but it is at least theoretically applicable to all sense-departments, and gives a much fuller explanation of the deviations from the Weber-Fechner law than do any of the others. [Like the volley theory, too, it combines certain features of both the frequency and the resonance theories of hearing, without involving the doctrine of the specific energy of nerves, the disproof of which has, in the opinion of many, put the latter theory out of count.]

[The most] recent work on the general lines just discussed indicates, for all but the highest intensities of sound, a linear relation between intensity of stimulus and magnitude of cochlear response. Wever and Bray (280), by the direct application of an electrode to the round window of the cochlea~~r~~ of a guinea-pig, measured the cochlear response to representative tones from 100 to 10,000 cycles. Plotting sound pressure in bars (P) against response in microvolts (E) on logarithmic coordinates, straight line curves (with the exceptions just mentioned) were obtained, which indicated a power function of the form

$$E = k\ P^{a}$$

in which a varied for different frequencies, usually with values round/

round about 1. The general conclusion is that the source of the Weber-Fechner function must be sought in processes beyond those revealed by the cochlear responses.

[I have not found it possible or, indeed, essential to give a fuller discussion of theories of sensory intensity in general, or auditory theories in particular, since it must be admitted that] experimental work in verification of any of these theories studies neural process and not sensation. On the other hand, Hecht (142) claimed to have fitted real introspectional sensations to an ogival curve, and Adrian (49) summarizes his work in a diagram (Fig. 59) which brings out the similarity between the course of sensation

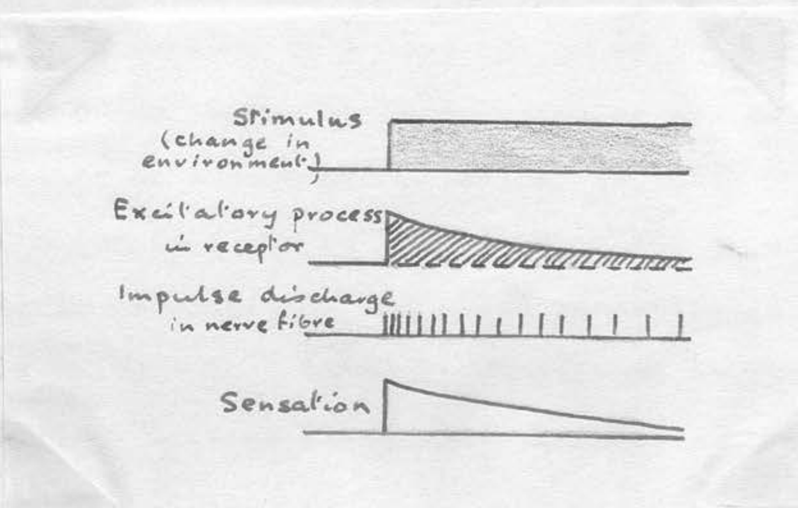


Fig. 59

Frequency theory of intensity

and that of excitation in the receptor. How far it is possible to equate sensation and excitation, nevertheless, is, as Boring (79) says, a matter on which we are not in a position to draw any definite inference.

On a still remoter plane from the actual phenomena of consciousness are attempts to study excitation in terms of ion concentration. Lasareff (168) and others have drawn up generalized laws of excitation on this basis for different sense-departments, including audition, but it is a problem on which only beginnings have so far been made.

Finally, there remains to be noticed an attempt by Graham (131) to treat intensity discrimination on behaviourist principles as a pure stimulus-response correlation. From this point of view the critical value of a variable stimulus is to be determined as a function of the other variables of the stimulating situation, and the problem is reduced to a description of the manner in which the complex of variables which constitutes the stimulus varies in the production of a constant behaviour effect. This approach makes a consideration in terms of conscious states or elements irrelevant to scientific description, and since sensation is in consequence presumably ruled out, I have not considered it necessary to deal with Graham's theory at any greater length.

VII. PRACTICAL APPLICATIONS OF SOUND INTENSITY DISCRIMINATION.

Certain practical applications of the differential threshold for sound intensity remain to be mentioned. They seem to fall under three main heads: (a) music, (b) clinical practice, and (c) industry. + noise-abatement.

Objective and subjective phenomena of sound intensity have numerous applications to musical theory and practice, e.g. the fact of the Broca phenomenon brings about a slight discrepancy in pitch between the first and second violins of an orchestra. figure-ground relationships and masking phenomena again are of considerable importance in problems of orchestration, and so on. More directly concerning my main topic is the conception of musical 'touch' as the power of voluntary control of intensity. Thus, Wickham (283) held that self-expression through intensity of sound rested primarily on intensity discrimination and precision in control of voluntary movement. Using a modified Seashore audiometer, a test consisting of intensity matching was given to a number of music students, and the results correlated with a ranking by the director of the college. The correlation was ~~possible~~ positive, though not significant, but correlation between rankings by director and instructor gave an almost identical figure (about + .3).

Alongside this result may be put that of Guernsey (13) who obtained her highest difference threshold from an oriental student without any musical experience. Hancock (140) found that musical ability/

ability lessened confusion in intensity discrimination, but not the amount of error. Andrews (52), however, makes no mention of intensity discrimination in his tests of musical talent. Yet judging from experience and verbal communications from a number of persons, I should think intensity discrimination is not without diagnostic value in this respect. How far it may be improved by training is another question point, and is touched on elsewhere.

As regards intensity discrimination in the partially deaf, it appears that this is not as a rule affected. This view is supported by Fremel (292) on the basis of experimental work with his measuring apparatus. Jones and Knudsen (154) definitely state that a lesion of the middle or internal ear has no effect on the capability of the cochlea to differentiate loudness. In fact, imperfect auditory capacities are now generally tested by modifications of the same procedures as are used in physical determinations of the limits of hearing, etc., and are based on the same partial agreement between loudness sensation and the decibel scale. A description of methods of testing along these lines was given by Fletcher (116) in 1926.

Changes in sound intensity are of the utmost significance in a great variety of industrial and allied problems. The majority have to do with machine noise, chiefly in respect of effects on the hearer, or of measurement and analysis with a view to abatement. Actual intensity discrimination has a more direct application in the/

the case of machines in which a fault in running causes changes in intensity, frequency, timbre, or any combination of the three. An apparatus for testing ability to distinguish and localize such irregularities was devised by Lintvareff (174). A somewhat similar problem was studied by King and Laird (159) who determined how one's accuracy in detecting the direction from which a sound comes is affected by noise present in the surroundings. It was found that the ability varied from moment to moment, and could be maintained at maximum for only a short time, by extreme concentration.

As regards the main group of problems indicated above, it is possible to make out a case for the measurement of noise in terms of its 'nuisance-value'. Earlier definitions of noise, in terms of lack of periodicity of vibrations, are beginning to be replaced by definitions such as that of Bartlett (59)*: 'Noise is any sound which is treated as a nuisance'. That it has not always been treated as such may be seen in the following quotation from a letter of James Watt (quoted by J.C. Prescott (<u>in</u> 5)):

"The velocity, violence, magnitude and horrible noise of the engine give universal satisfaction to all beholders . . . The noise seems to convey a great idea of its power to the ignorant, who seem to be no more taken with modest merit in an engine than in a man."

To this we may add the finding of Vernon and Warner (260), whose experience it was that industrial workers seldom objected to noises, even if very loud, especially in purely manual work. On the other hand/

*

hand, the same investigators found a decrease of efficiency in simple mental work under conditions of noise.

Much work has been done on problems of this sort, and it has been noted (by Fletcher, quoted in (291)) that to the average individual, a noise composed of widely separated frequencies is less disturbing than one of equal intensity but having components close together in the frequency range.

A report of a practical problem studied partly under non-laboratory conditions was made by Geiger and Abbott (128).

In the first part of the experiment 21 observers (14 male; 7 female) rated six vacuum cleaners for loudness and disagreeableness. The observers were allowed to switch on and off freely. The sounds were also studied by noise-meter measurements and frequency analyses. It was found that while there was a high degree of disagreement among individual observers, the composite ratings for both loudness and disagreeableness agreed almost perfectly with the objective order of loudness. The result is the more surprising in that no individual rated for loudness and disagreeableness alike. The conclusion is that while individual observations have little meaning, the average of a group of observers is a reliable criterion of loudness.

This result was borne out by further experiments, on similar lines, but with more carefully controlled conditions. Two series of comparisons of sounds were made by 20 observers. A large sheet-iron reflector was rotated to shift the sound-wave patterns so that an average could be obtained. In the first series six miscellaneous/

miscellaneous sounds were rated in the same way as the vacuum cleaners, and again a perfect check with the meter readings was obtained. Differences of less than 1 db were correctly placed - this being less than the difference threshold. In the second series the same six sounds and five others were balanced against a 1000 cycle test-tone. The sounds varied in db level from 40 to 76 db, and while individual observers made mistakes up to 20 db (though usually less) the average discrepancy over all varied between - 0.2 and + 4.4 db.

The authors lay considerable stress on the accuracy of the average judgements, but it is also interesting to note the immense individual variation, especially in view of certain researches (especially the earlier ones) which were carried out with a small number of subjects.

It is therefore evident that noise may to some extent be measured by its subjective effects, but Richards (216) and others have pointed out that adaptation to continuous steady noise even of high intensity may take place. On the other hand, such a method of measurement is obviously imperfect and limited, and if it is desired to achieve abatement to any given extent, some physical method of measurement, such as those earlier described must be employed. It is here that the practical importance of the work on Loudness measurement described in Section V, and, indeed, of all work on intensity discrimination, may be seen.

An example in point may be seen in the work of Weston and Adams/

Adams (276), who investigated working conditions in a weaving shed with a noise level of about 96 db. The use of ear-defenders which could reduce this level by 15 db was advocated. The authors show that while this reduced the intensity to less than one-thirtieth of its original value, the apparent effect, as seen in loudness decrease was much less, being more nearly equal to a reduction of about one half. This agrees with the loudness scale drawn up by Churcher, King, and Davies (5). Ham and Parkinson's (16) scale would make it a decrease to about one third.

Weston and Adams show that the significance of noise is conditioned by differences of temperament. This is probably as far as it is possible to go at present, although there is at least a grain of truth in Herbert Spencer's statement (quoted by Richards (216)) to the effect that it is possible to gauge a man's intellectual capacity by his intolerance of unnecessary noise. This might conceivably be verified experimentally, but it would hardly be a profitable enquiry.

VIII. EXPERIMENTAL.

Before describing my own experimental work, I wish briefly to discuss certain points with respect to method.

In drawing up my experimental programme, which, as explained in the Introduction, was necessarily limited, I followed two main guiding principles.

First, since the chief purpose of my experimental work has been to check for my own satisfaction the findings of others, rather than to establish original results, I have devoted more attention/

attention to the less orthodox methods, in preference to those which have been thoroughly standardized and established. This explains my choice of methods of estimation and of mean gradation for my comparatively extended experiments, in spite of their reputed low degree of reliability.

Second, I have not attempted to obtain data allowing for a full application of the Constant process, since I believe that Irwin (150) is right in stating that few collections of data are ever obtained which are worth the refinement of the constant process. Shaad and Helson (225) claim that 5 stimuli and 100 series (500 observations in all) are the lowest numbers that can safely be used, and Cowdrick (101) also stresses the necessity of studying the validity of the Weber-Fechner law with longer series. But such extended experiments usually require that the observation periods be distributed over two or more days, and my own experiments have suggested that this may lead to complications. (I return to this point later.) Various writers have shown that the number of series required varies with the individual, especially with regard to his degree of practice. Boring (72) shows that the number also varies with the use to which the threshold is to be put, and that in many cases a difference based on only ten series is highly significant.

The difference between thresholds obtained by different psychophysical methods is a topic which has attracted much attention since early times. Mosch (182) in 1902 developed a formula relating Limits thresholds with the precision measures of the method of Right and Wrong Cases. Kellogg (22) much more recently compared the methods of 'constant stimuli' and 'average error' on the basis of/

of their respective variable errors, and decided that the former was the more reliable, but that the latter gave the finer measures of differential sensibility. Interpolation procedures were recommended by Newhall (190, 191) as worth the saving in time, and not essentially different in their results from the constant process. The most recent reviews are those of Culler (104) and Irwin (150) already referred to in other connections. Irwin concludes that sensory differences are not measured by a sense-organ at all, but by a statistical classification of the organism's responses. This is a point I have already stressed elsewhere. A more comprehensive statement may be adapted from Fernberger (114), who shows that the old idea that we are measuring the sensitivity of a particular sense-organ has been abandoned in favour of the view that we are measuring the sensitivity (or variability of response) of the entire organism, including the sense-organs, to a given stimulus situation, as influenced by varying conditions of concentration, attitude, acceptance and comprehension of instructions, practice, and so on.

In my experiments, therefore, I have attempted to keep these conditions as constant as possible, while recognizing that many factors are almost entirely outside the control of the experimenter.

As regards the experiments on the estimation of loudness, I have been guided in general by the methods described in Section V, and more particularly by that of Richardson and Ross (39). Although these methods are of comparatively recent development, and definitely/

definitely unorthodox from the point of view of established psycho-physical method, they are related in certain ways both to the normal methods, and to that of Single Stimuli (or 'Absolute Judgement') recently proposed and standardized by Urban (259), Volkmann (264), and others. Bressler (84) described certain lifted-weight experiments in which the subjects actually estimated the stimuli in absolute units, which were later translated into the usual three categories. The method I have used seems to me to be intermediate between this and that of Right and Wrong cases, or perhaps Keller's <u>mehrfachen Fälle</u> (20). In the case of some observers, too, who report the imaginal setting up of an auxiliary standard, it approximates to the comparison with a mental standard discussed by Woodworth and Thorndike (285) as far back as 1900.

Eight subjects took part in the experiments:

A. Mr. I.A. Gordon, University Assistant in English.

B. Mr. B. Semeonoff, (the present writer), University Assistant in Psychology.

C. Mr. K.J.W. Craik, graduate student in Psychology.

D. Mr J.D. Dalgliesh, undergraduate.

E. Mr. E.K. Stiles, undergraduate.

K. Miss M.J.O. Kennedy, undergraduate.

M. Miss M. McGregor, undergraduate.

R. Miss M.M. Robertson, undergraduate.

The five last-named were all members of the Ordinary Class of Psychology, and had had some experience of psychological experiments in general - D and R considerably more than the others. None of the subjects had had special training or practice in making intensity judgements. The only one of the subjects having a definite 'musical characteristic' (see Sabine (221)) was C (violin). All the subjects had normal hearing, and were in normal health.

The subjects participated in the various experiments as follows:

Watch-tick: B, K, M, R.

Fall-phonometer: B, D, E.

Tuning-fork - Limits: A, B, C, D.

Mean gradation: B, D.

Loudness estimation: A, B, C.

Watch-tick experiments.

Since determinations of differential thresholds using a watch as source of sound had apparently not been made since the pioneer work of Renz and Wolf (38), I decided to carry out a series of experiments under slightly different conditions.

In the first place, Renz and Wolf found only a lower threshold, and used only one intensity. The present experiments cover four intensities, and both upper and lower thresholds. Secondly, Renz and Wolf's determinations were for uniaural hearing; mine are for binaural hearing. Finally, an improvement was made in the type of watch used. Renz and Wolf used an ordinary watch, and had to screen the sound when necessary by means of a piece of cardboard. In the present experiments a stop-watch was used, of a pattern which started and stopped without any extra click.

Four subjects took part in the experiment. K and M worked between 9 and 11 a.m., B and R between 2 and 4 p.m. The subject was seated with his back to the source of sound. No head-rest was used, but the shape of the chair made it possible to maintain a constant position without difficulty, and frequent checks on head-position were made. A heavy laboratory table was placed immediately behind the subject's chair, on a level with his ears. On this a strip of paper was pinned, with the standard and variable distances marked. The validity of the inverse square law was assumed, and points were chosen to represent intensities in the ratio of 1.4: 1.2: 1 : 0.8 : 0.6. The choice of these values was determined by a few preliminary trials by the method of/

of limits. The experimenter held the watch in one hand, which rested on the table, in such a way that the watch was in direct line with the subject's ears, and that the minimum of resonance was given. In spite of all precautions, however, all the subjects reported fluctuations in intensity, which may, however, have been due to subjective causes. The experiments were carried out about the middle of a partially sound-proof room, but even so extraneous noises had a considerable distracting effect.

Four standard distances were used - 50, 70, 100, and 200 cm., giving intensities in the ratio of approximately 0.25: 1.0: 2.0: 4.0. The first two of these are perhaps below the range within which the inverse square law may be expected to be valid, and only a few observations were made with the standard of 50 cm. The results at 70 cm., however, seemed to be as reliable as those at the two greater distances. Both time-orders were used, and the total number of presentations of each variable for each subject were as follows: 50 cm. - 10; 70 cm. - 30; 100 cm. - 40; 200 cm. - 20 for K and M, 30 for B and R. Each stimulus was sounded for about 2 seconds; 25 pairs of stimuli constituted a series.

Considerable difficulty was encountered in obtaining the thresholds given in Table XXVIII, owing to the occurrence of a number of reversals in the results. Calculations by Spearman's formula (295) gave very low thresholds for this reason; in fact, in some cases a lower threshold was found above the standard, and/

TABLE XXVIII.

Standard. cm.	Upper		Lower		Upper		Lower.	
	SV	VS	SV	VS	SV	VS	SV	VS
	K				M			
50	.23	.32	.51	.23	.30	.15	.27	.27
70	.56	.63	.32	.37	.48	.19	.29	.17
100	.44	.34	.37	.36	.20	.40	.19	.15
200	.60	.26	.60	.48	.26	.20	.31	.13
	B				R			
50	.30	.23	-	-	.34	.23	.25	.15
70	.31	.25	.34	.36	.13	.23	.26	.15
100	.13	.29	.25	.40	.12	.15	.14	.13
200	.38	-	.44	.32	.30	.38	.25	.29

and vice versa. Eventually I decided to find 'Fullerton-Cattell' thresholds, by interpolation, and, when necessary, extrapolation. Judgements of 'equal', according to the procedure usual in this case, were distributed among the 'greaters' and 'lessers', and the value yielding 75% of the required type of judgement taken as the threshold. Only in this way was it possible to utilize all the data available.

In Fig. 60 the upper and lower thresholds with both time-orders for each intensity are averaged. It will be noticed that discounting the highest intensity, for which the thresholds are based on a total of only ten observations of each variable, the curves tend to follow a similar course in each case, i.e., first down/

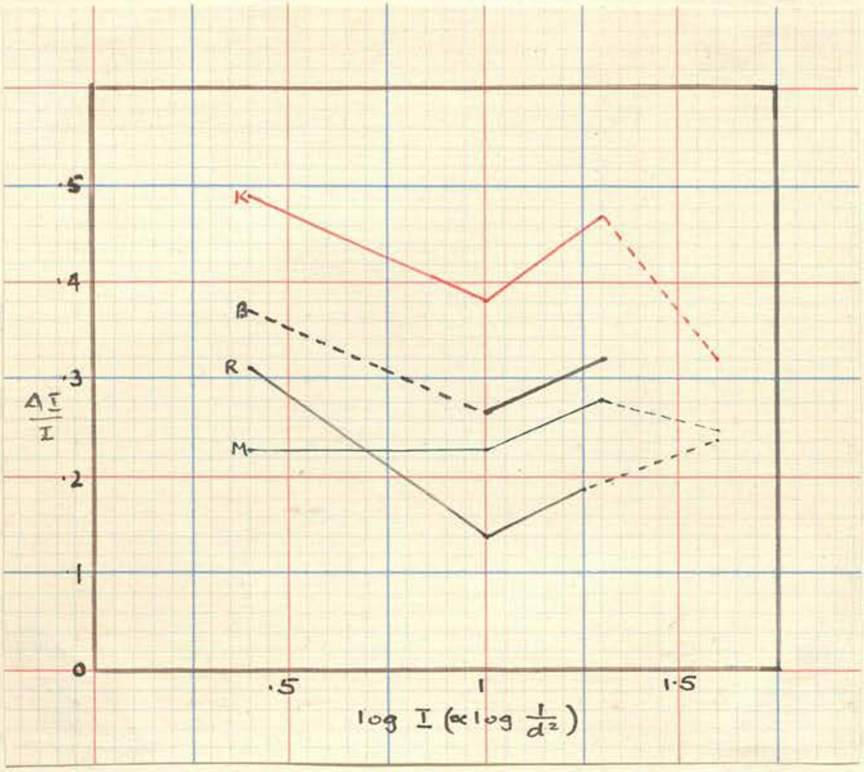


Fig. 60

down and then up. The initial drop seems to suggest that at very weak intensities the value of the threshold tends to rise, while the subsequent rise perhaps indicates that 70 cm., as well as 50 cm., is too small a distance at which to apply the inverse square law. On the other hand, as already noted, the various values of the threshold for each observer at 70 cm. show just as much (or just as little) constancy as at the higher values.

Additional conclusions may be drawn as follows:

(i) Weber's law does not appear to hold for watch-ticks of different intensities, since values of the threshold approaching constancy/

constancy over the range studied were obtained with only one observer (M) out of four.

(ii) Very large individual differences as regards discrimination were found.

Phonometer experiments.

Three subjects (B, D, E) participated in a short series of experiments with Wundt's Fall-phonometer (see Fig. 3, p. 24). The experiments were carried out between 2 and 4 p.m. on three different days.

Two ivory balls were used, weighing respectively 14.225 and 14.30 gm. This difference (c. 0.035%) could safely be neglected, and in the conduct of the experiment the balls were changed round indiscriminately. The base-plate was of zinc and measured 70 x 10.5 x 0.25 cm. The plate was laid on a strip of felt nailed to a wooden box, and was inclined at an angle of about 20° to the horizontal. The balls were released electrically by means of a contact pendulum, the time interval between the breaking of the two contacts being 1.5 seconds. The balls were caught on the rebound in a cloth 'apron'. Only the two centre sections of the phonometer were used. At the half-way point in the experiment the connections were interchanged, so that the order of fall was reversed. The subject sat at a distance of 2 metres, with his head lightly held in position as shown in Fig. 61.

Preliminary determinations by the method of limits with a standard/

Fig. 61

standard height of 120 cm. gave upper and lower thresholds of about 15 cm. and 12 cm. respectively. These values, however, were obtained from readings which included a number of reversals, and it was therefore decided to try the method of Right and Wrong cases, using a fairly large interval between the values of the variables, and determining only a lower threshold.

Since it was thought desirable to make use of the categories 'greater' and 'less' in about equal proportions, and since only a lower threshold was being investigated, some arrangement was necessary to combine both time-orders. The difficulty of manipulation of the apparatus was too great to admit of constant interchange of standard and variable, so a method of 'group presentation/

presentation' analogous to but less drastic than that of Shaad and Helson (225) was adopted. Thus, unit II of the phonometer was set at a given height, and a number of stimuli were presented with this as standard and the variable at different heights on unit III. The standard was then set on unit III, and the variable on unit II, thus giving the reverse time order, which was retained for another small group of presentations. The procedure of changing over was then repeated. Each group consisted normally of from 3 to 7 pairs of stimuli; a complete series consisted of 28 pairs. In this way both units served as standard and as variable within each series. A further permutation was later obtained as noted above.

Two standard heights were used: 150 cm. and 120 cm. Each was presented with itself and with three variables at intervals of 15 cm. Smaller heights were found to be impracticable, since at a height of about 60 cm. the balls did not rebound sufficiently to clear the plate. In all, 20 presentations of each variable were given to each subject, except in the case of the standard with itself, which was given 10 times.

In view of the conflicting opinions on the relation between height of fall and intensity of sound expressed by the early experimenters (see pp. 19 ff.) it was decided to make an attempt to determine this relation roughly by means of a Piezo-Electric microphone used ballistically. The extent of swing of the ammeter needle was noted, and intensity was taken as proportional to the square of the voltage. Four heights were used, in the ratio 1 : 2 : 4 : 8 and a modal value of the readings obtained

was/

was taken in each case. Analysis of the results suggested 1.07 as the most probable single value of ε in the formula

$$i = ch^{\varepsilon}.$$

(The weight of ball, being constant, could be neglected). This result is in direct contrast to those of Tischer (44) Merkel (31) etc., who obtained values of ε less than 1. It must be emphasized, however, that 1.07 is in no sense to be regarded as a definitive value, since the experimental values obtained at the different heights varied up to 10%, and since the value 1.07 was deliberately chosen as a best value to fit all four heights, whereas ε itself probably bears some functional relationship to h.

Thresholds were calculated using Spearman's formula (295). The choice of formula was determined partly by the comparatively small number of observations, and partly by the occurrence of a few slight reversals in the data, which made interpolation impracticable in a few cases. In making the calculation the judgements on the standard with itself, which were fewer than on the other variables, were not included, and since these contained a few judgements of 'less', the thresholds should probably be a little finer than the figures quoted. The thresholds given in the accompanying table are based on the assumption that ε = 1.07 gives a truer value of intensity. If intensity is taken as directly proportional to height, the threshold values are reduced (throughout the range represented) by about .01.

TABLE XXIX./

TABLE XXIX.

Subject.	Spearman thresholds.			
	Standard 150		Standard 120	
	SV	VS	SV	VS
B	.21	.25	.26	.21
D	.18	.16	.20	.13
E	.18	.15	.24	.13

The following conclusions may be made from the results of this experiment:

(i) The use of the fall-phonometer does not appear to be a reliable means of determining difference thresholds for sound intensity. Differences of timbre were found to be almost impossible to eliminate, and in view of this it would seem that the trouble necessary to determine an accurate relation between height of fall and effective sound energy is hardly justified. Both subjective observation and microphone readings suggest that the intensity (and loudness) produced under apparently identical conditions fluctuates from moment to moment.

(ii) The experimental results suggest that there is a fair degree of individual difference in sensibility as measured by this means. Since only two intensities were investigated it is impossible to make definite conclusions as to the constancy of the threshold, although the values obtained are fairly close to one another. The results are best represented in general terms by an overall average of 0.2. This lies between the values obtained by the Philosophische Studien group on the one hand, and by/

by Keller (20) on the other.

(iii) The occurrence of a negative time-error (second stimulus stronger) is fairly pronounced, since it occurs to a varying degree in five cases out of six.

Tuning-fork experiments.

As already stated, both the watch and the phonometer proved rather unreliable as sound sources. In addition, they have the disadvantage that it is extremely difficult to assess their loudness in db units. Accordingly, the greater part of my experimental work was carried out with a valve-maintained tuning-fork.

The instrument consisted of a 512 cycle tuning-fork rigidly fixed on a metal base. Close to one arm was a permanent magnet with polepieces having 2000 ohm coils, taken from wireless headphones, and next the other arm, a single 1000 ohm maintaining coil with a soft iron core. Leads from the magnet coils went to a commutator and the input side of a three valve amplifier. In series with the anode output circuit was the maintaining coil of the fork. The minute vibrations which the fork always made were sufficient to induce a current in the magnet coils which was amplified, passed through the maintaining coil, and, when the commutator was put over the correct way, reinforced the vibrations until they assumed an equilibrium amplitude when the maximum maintaining magnetic force balanced the force required to deflect the tuning-fork arm through a certain distance. There was thus no mechanical interference with the fork, and no contacts to burn.

Across/

Across the anode resistance of the maintaining amplifier were insulating condensers leading to the input side of a single power-valve amplifier which had its own set of batteries. The two amplifiers were separately screened. Alterations in the volume by control of the output amplifier were thus prevented from affecting the maintaining current for the fork. Resistance-capacity coupling was used to reduce distortion. The first harmonic of the fork, very plain when it was struck by hand, was almost unnoticeable when the fork was electrically maintained and amplified. A moving coil loudspeaker was used, giving a maximum loudness, at a distance of four inches, of approximately 84 - 90 decibels above the threshold, and a constancy ~~of~~ within 0.5%. The tuning fork was covered with a glass case having a padded rim, both to render it inaudible and to avoid the damping effects which were found to occur if it was exposed to the air waves from the loudspeaker.

For varying the speaker volume shunt-and-series attenuators were used, having values of - 60 decibels fixed and - 60 decibels variable in steps of 2 db, giving a total of 120 db attenuation, and voltage control at the power-valve grid. Neither was perfectly dependable without calibration by a microphone placed in the position which the subject's ear is to occupy.

In switching off the sound three methods were employed - breaking the speaker circuit, shorting the power valve grid and filament, and shorting the speaker. The first caused annoying clicks, even when using condensers and chokes, owing to the making and breaking of the H.T. circuit. The second was much better, but any interference with a grid circuit is apt to introduce small noises/

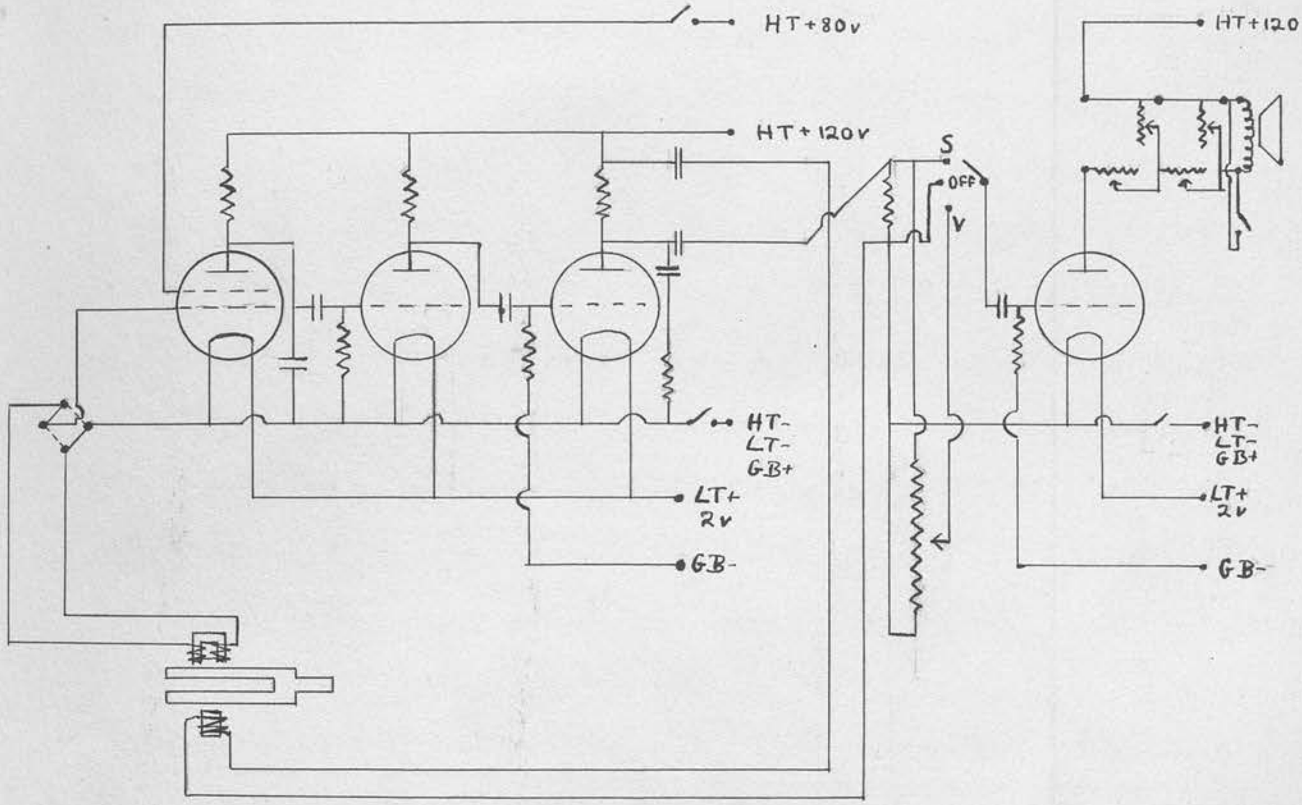
noises. The third proved the best method. The mechanical parts of the switches, etc., were made as silent as possible.

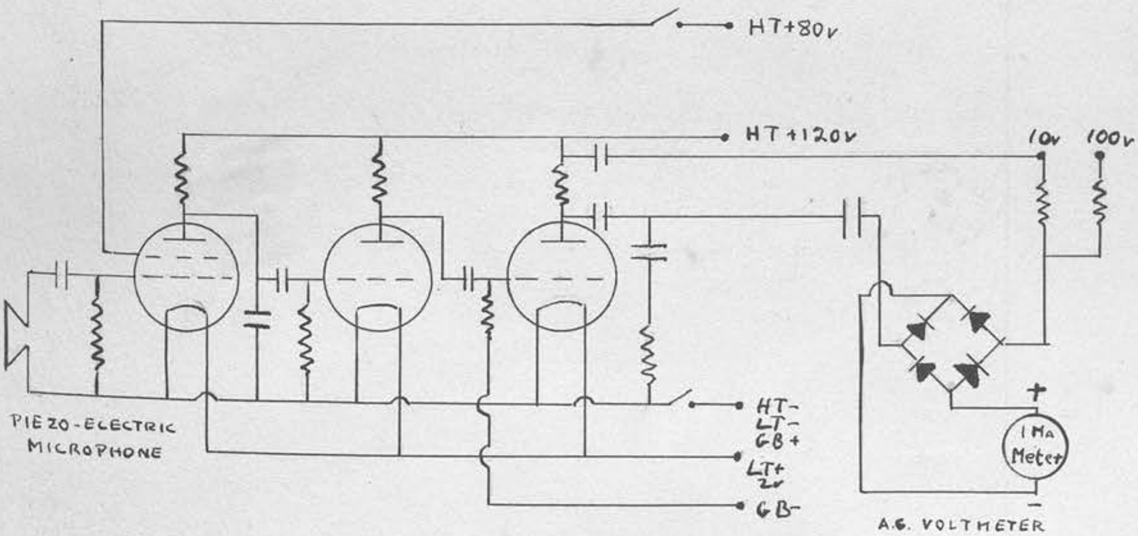
Besides using the attenuators to give different intensities, the grid potentiometer of the output valve was calibrated by a microphone used with a three valve resistance-capacity coupled amplifier and an output voltmeter. This latter consisted of a 1-Milliamp full scale moving coil meter, Westinghouse instrument rectifier, insulating condenser, and metallised resistances giving 10 and 100 volt A.C. scales. There is a slight voltage drop in the rectifier which only affected the first half volt of the readings. This enables the voltages to be read with an accuracy of 1 or 2%. The physical intensity of the sound (taken to be proportional to the watts) is equal to the square of the voltages, or in decibels, $20 \log \frac{V_1}{V_2}$.

Two types of microphone were used - a carbon one after the Reiz pattern with a transformer and a Piezo-electric one utilising a "bimorph" element of Rochelle salt. The latter has the advantage of requiring no battery, polarising voltage or transformer and having a voltage output proportional to the amplitude, and not the velocity, of the deflection.

A wiring diagram of the apparatus is given in Fig. 62, and a photograph in Fig. 63.

VALVE MAINTAINED TUNING FORK





MICROPHONE & VOLTMETER

K.J.W.C.

Fig. 62

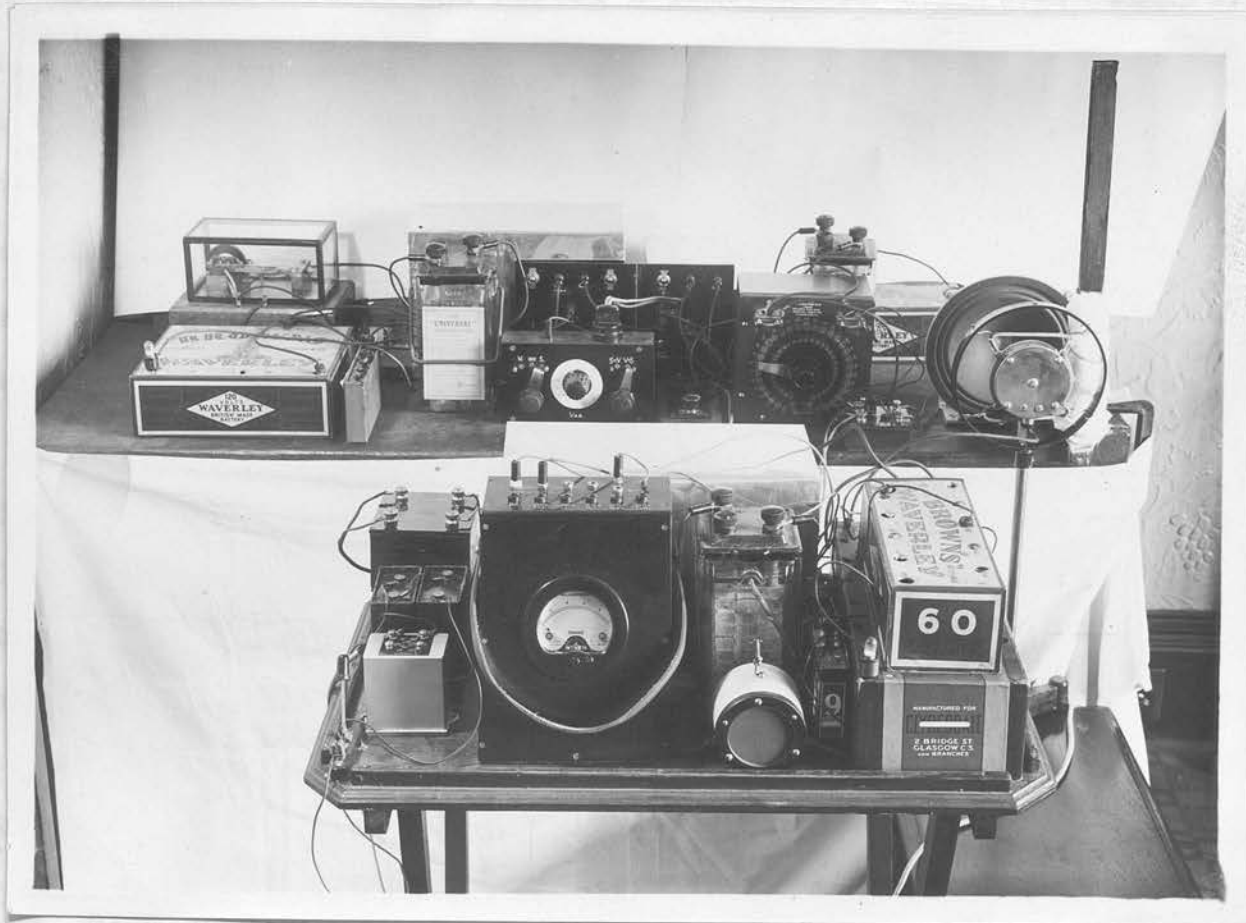


Fig. 63

Tuning-fork and calibration apparatus

The instrument was calibrated with the microphone at a distance of 4 inches from the loudspeaker, and during the experiments the subject sat with his left ear at the same distance. The subject's head rested against a flat vertical wooden bar covered with a thick linen pad. The other ear was left unstopped. In general, the practice throughout all the experiments was to allow the subject to assume/

assume as comfortable and normal a position as possible. Accordingly no such devices as blindfolding the subject, using elaborate head or chin rests, etc. were employed, as it was thought that discomfort thus caused might outweigh any advantage obtained by controlling the external conditions by such means. At the same time, a close check was kept on any possible alterations of position on the part of the subject, and corrections had seldom to be made.

All the tuning-fork experiments took place between 5 and 7.15 p.m., and extended over the period from late February to early May, 1936. The period of stimulation in the various methods varied between 1.5 and 2 seconds, and the interval between standard and variable was .5 to 1 second.

Method of Limits.

Four subjects (A, B, C, D) were tested for lower thresholds by the method of limits. Each (except A) judged at 9 intensity levels, from - 80 to 0 db attenuation; A, whose discrimination was rather coarser than that of the others, omitted the lowest intensity. Taking the absolute threshold at four inches for the 512 cycle tone produced by the tuning-fork as - 90 db attenuation, the intensity range covered, as defined by the standards of the American Standards Association (see p. 157), is approximately 25 - 105 db intensity level.

A complete limits series consisted of an ascending and a descending series in the time-order SV (standard followed by variable), and the same in the time-order VS (variable followed by standard). Thus an average of four readings determined the threshold at/

at each level. The variables were graded by means of the grid potentiometer, which was calibrated in fractional units at intervals of 0.05, ranging from 0.4 to unity. Unfortunately it was not found possible to obtain more than three complete series with subjects B and D, and two with subjects A and C, owing to some preliminary difficulties in calibrating the potentiometer. On the other hand, each subject gave several complete series of observations with the tentative calibrations, and these both served for practice, and yielded results of the same general nature as those on which the results quoted below are based.

Figs. 64 to 67 show the complete results for each subject. In each figure each type of line (continuous, dots, or dashes) shows the course of the threshold over the range for one complete series. The heavy line indicates a smoothed curve based on the average threshold for each intensity. In general I have followed these points, indicated by discs, fairly closely; the biggest divergences of the smoothed curve from the observed points occur in the case of subject B. These average thresholds are also given in Table XXX.

Fig. 64
Subject A.

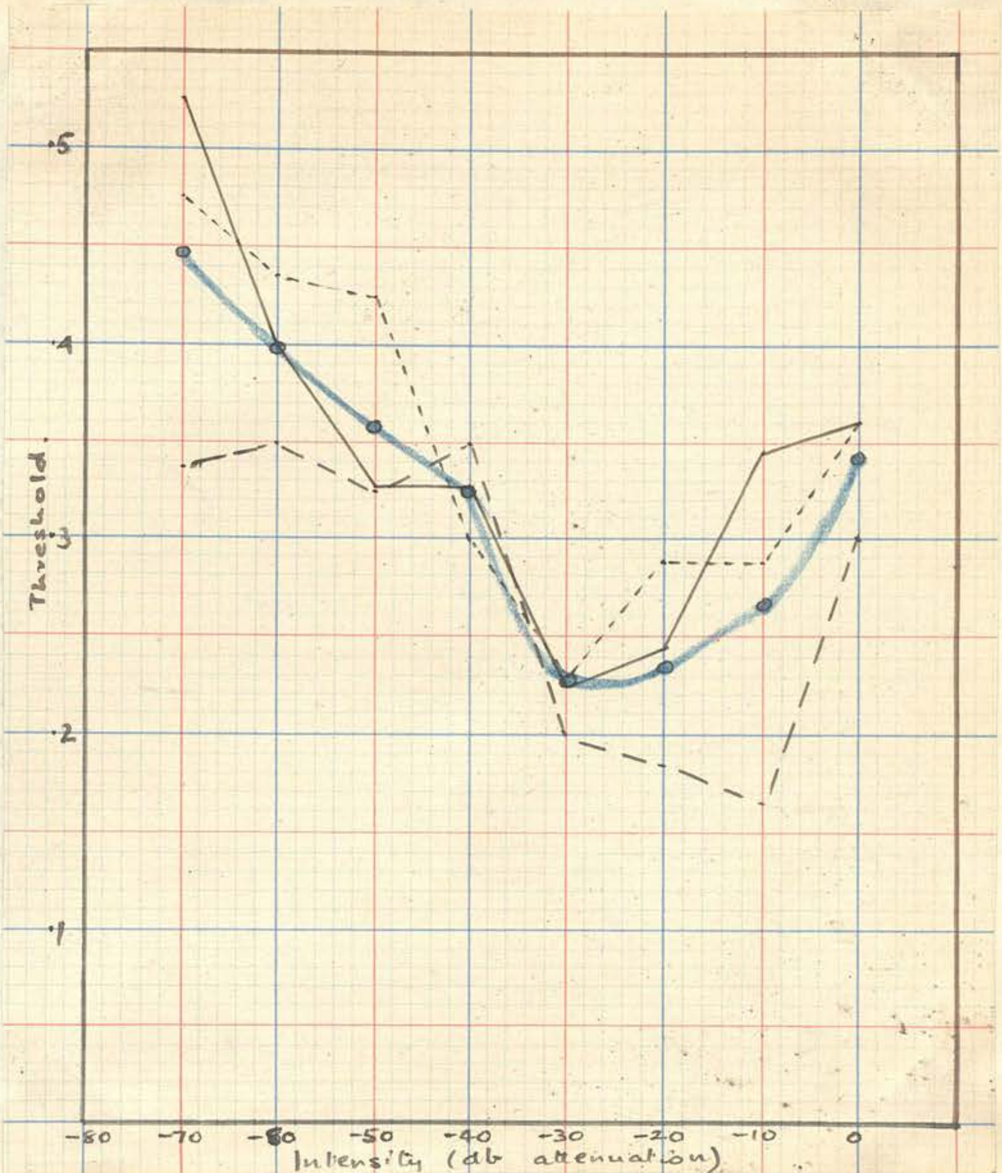
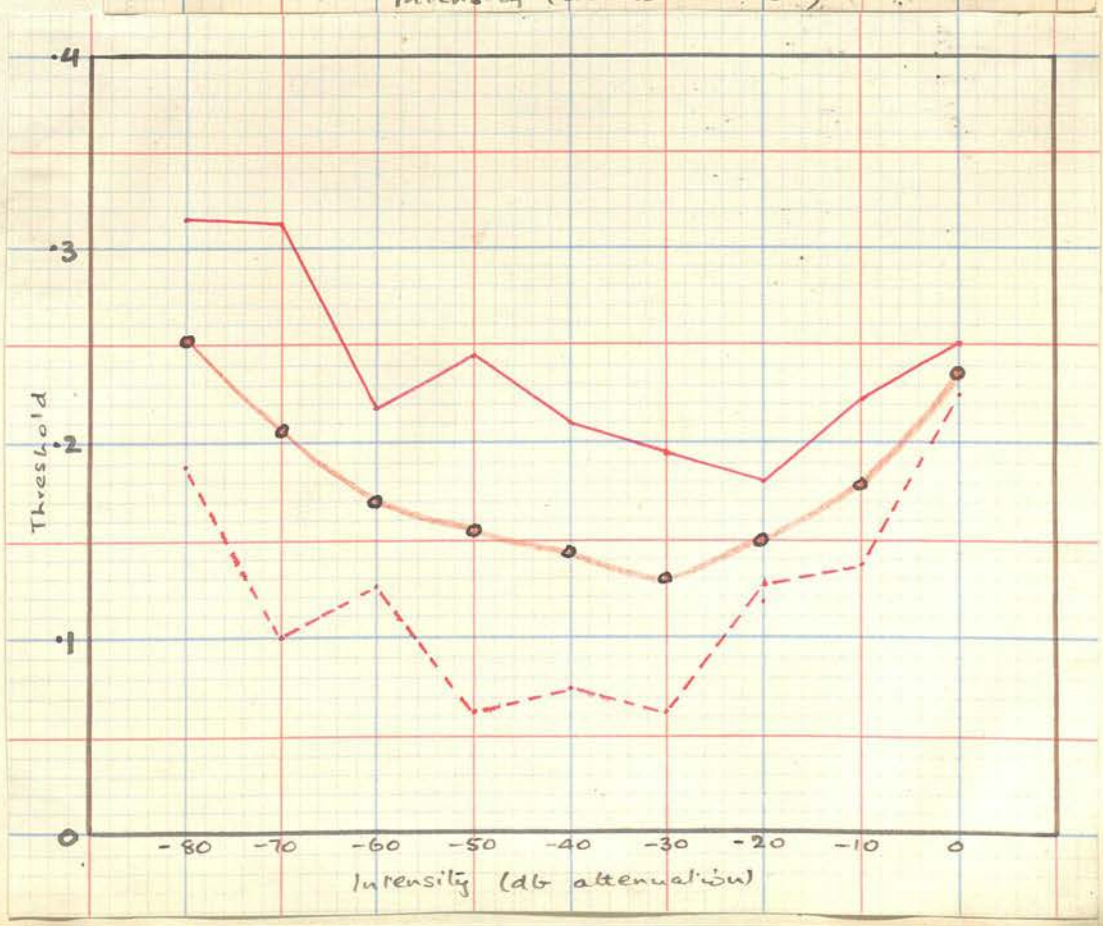


Fig. 65
Subject D



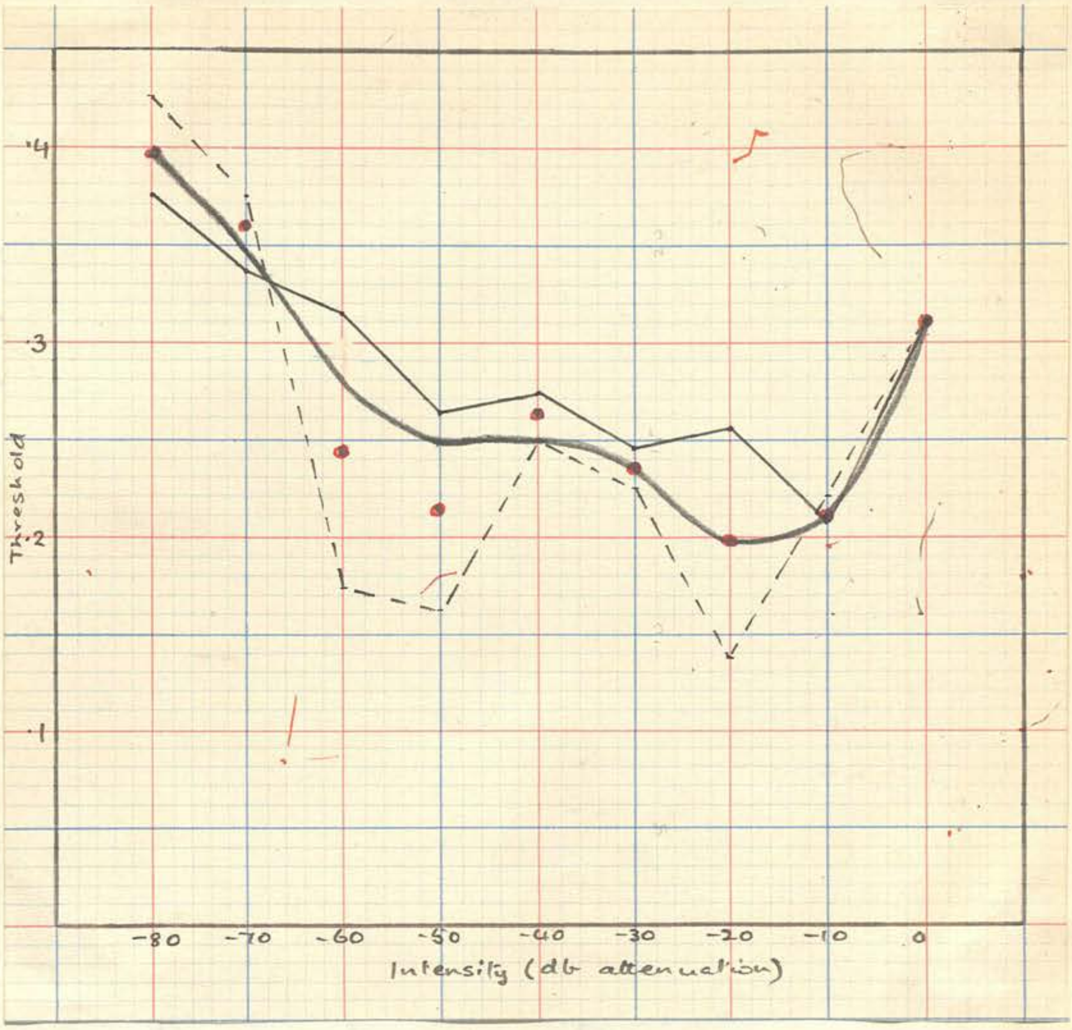


Fig. 66
Subject B

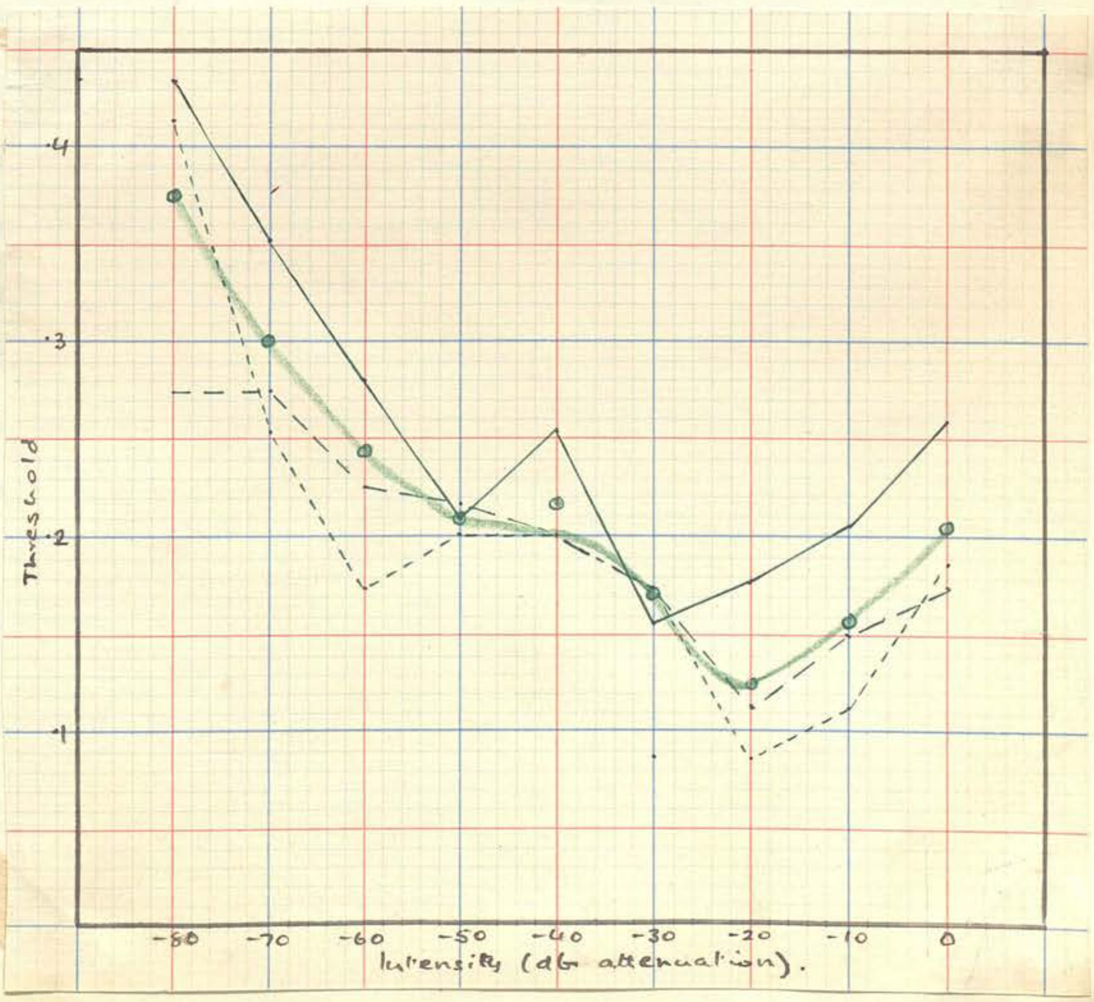


Fig 67
Subject C

TABLE XXX.

Db attenu-ation.	Subject and number of series.			
	A (3)	B (2)	C (3)	D (2)
.0	.342	.310	.208	.238
-10	.264	.211	.156	.180
-20	.235	.197	.126	.151
-30	.224	.235	.177	.129
-40	.326	.261	.218	.143
-50	.359	.213	.208	.154
-60	.396	.245	.244	.171
-70	.446	.358	.297	.206
-80		.400	.375	.251

The smoothed average curves of Figs. 64-67 are shown superimposed in Fig. 68. A consideration of all five figures suggests the following conclusions:

(i) Weber's law does not hold, even for a limited range, in sound-intensity. The value of difference threshold is higher at the extremes than in the middle of the intensity range, and varies continuously throughout.

(ii)/

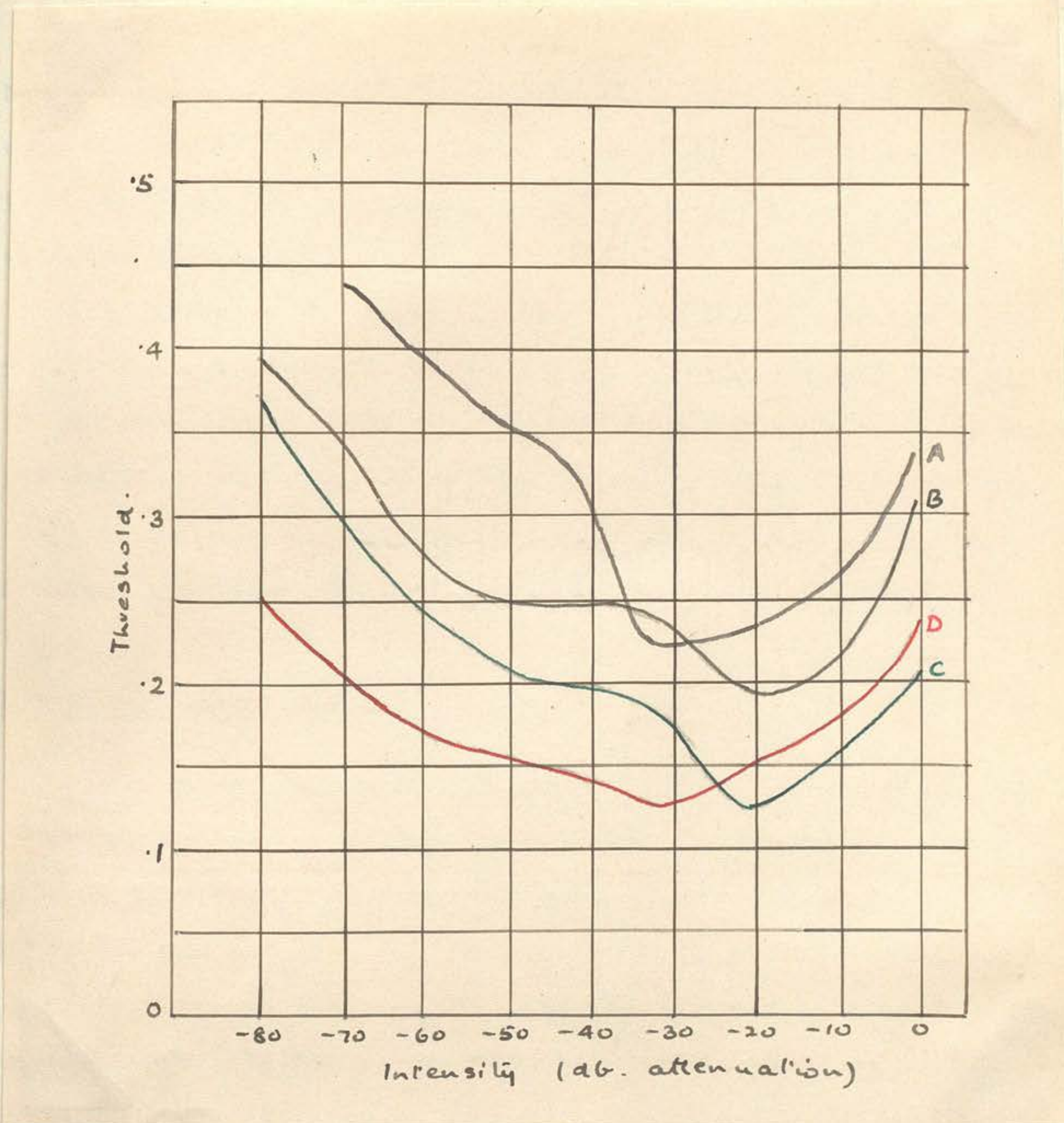


Fig. 68

(ii) The very tentative results obtained suggest that this decrease is at first rapid, then more gradual; after this another rapid decrease may appear, followed by a final increase, to a value probably not as high as with very weak intensities.

(iii) Considerable individual differences exist, with respect both/

both to fineness of discrimination, and to the shape of the curve obtained, though this difference is less marked than the first. In particular, the point of greatest sensitivity seems to vary with the individual.

(iv) Although the curve showing course of threshold remains fairly constant for each observer, great variations of sensitivity to difference occur from day to day; cf. in particular, the results for subject D. The use of the method of limits may have something to do with this phenomenon, and possibly a check by the Constant method might yield different results.

Method of Mean Gradation.

A short discussion of the method of mean gradation is given in my account of the work of Merkel (32) and Angell (3). The present experiments show the following main differences from those of Merkel and Angell: (i) the substitution of tuning-fork tones for phonometer noises; and (ii) the use of terminal stimuli having a considerably higher ratio than those investigated by Merkel and Angell. Other differences will be noticed in the course of the following account of the experiments.

Two subjects (B and D) took part in these experiments. Each judged an estimated mean between stimuli having intensity ratios of 1 : 10 and 1 : 100 respectively; i.e., at intervals of 10 and 20 db. Each ratio was presented at three intensity levels, and both time-orders were used, i.e,

softer sound - variable - louder sound (SL)

and louder sound - variable - softer sound (LS)

For/

For the ratio 1 : 10 the terminal stimuli (in readings of the variable attneuator) were - 10 and 0, - 30 and - 20, and - 60 and - 50. In each case 4 variables at steps of 2 db were used. For the ratio 1 : 100 the terminal stimuli were - 20 and 0, - 40 and - 20, and - 60 and - 40. Normally 6 variables (occasionally 5 or 7) at steps of 2 db were presented. In all cases the highest variable was greater than the arithmetic, and the lowest less than the geometric mean. The choice of first and third intensity levels was determined by their being the highest and lowest values represented on the variable attenuator. The region - 40 to - 20 was chosen as being that in which sensibility, as shown by the Limits experiments, was finest. Intensities below - 60, obtained by means of the fixed attenuator demanded rather too close attention to make their use feasible.

An attempt was also made to use the grid potentiometer to investigate ratios of about 1 : 2. It was also impossible, however, to work at this ratio with any useful degree of accuracy, and the experiments were discontinued after a few trials. Much the same occurred in the case of ratios of more than 1 : 100: if the variables were spaced far apart the task became too easy; if close together, the subject stated that he was only guessing. Accuracy might perhaps have been attained as the result of practice, but I did not consider this worth while, and decided to concentrate on the two ratios described. The observations thus made, however, served for practice, and in addition, practice with the actual values used in the main experiment was given. Taking both time-orders together, each subject made about 500 judgements on these values.

The/

The estimated means are given in Table XXXI. The figures quoted are in terms of multiple intensity units, assigning the value 1 to the lower standard in each case. These were obtained by distributing the 'mid-way' judgements among the two other categories, and interpolating a db value corresponding to 50%; this was then converted to intensity units by reference to anti-logarithm tables.

TABLE XXXI.

Terminal stimuli.	B		D	
	SL	LS	SL	LS
Ratio 1 : 10		A.M. 5.5	G.M. 3.16	
- 10 : 0	3.63	2.35	3.44	2.69
- 30 : - 20	2.86	3.07	2.99	2.78
- 60 : - 50	2.51	3.49	3.29	3.16
Ratio 1 : 100		A.M. 50.5	G.M. 10	
- 20 : 0	33.1	15.9	28.2	13.2
- 40 : - 20	20	13.2	13	17.6
- 60 : - 40	13	14.7	30.2	13

As will be seen, considerable divergence occurs between the results of the two time-orders. This discrepancy, however, is never more than 17% of the upper standard, and usually, especially in the case of the ratio 1 : 10, considerably less. The results for subject D show a higher mean for the time-order SL five times out of six; in the case of B, however, they are equally divided.

As/

As regards the relation between the arithmetic, geometric and estimated means, a first inspection reveals that in the case of the ratio 1 : 10, at least, the estimated mean is closer to the geometric than the arithmetic mean; in fact in one case it coincides with the geometric mean, and in seven of the remaining eleven cases it falls below the geometric mean, and is thereby ipso facto nearer to it than to the arithmetic mean.

If, however, Merkel's formulae

$$F_g = \frac{m}{g} - 1$$

$$\text{and} \quad F_a = \frac{m}{a} - 1$$

(where F_g and F_a are deviations of the estimated mean m from the arithmetic mean a and geometric mean g) are applied to the results for the ratio 1 : 100, complications arise.

Merkel's formulae must be assumed to be based on the following argument: Since a is greater than g, a value of F_a numerically equal to a value of F_g is relatively a smaller deviation from a than a value of F_g equal to F_a is from g. Accordingly to obtain a truer comparison of F_a and F_g one must consider not their absolute values, but their values relative to the quantities from which they represent deviations.

Now, if m assumes a value equal to or greater than a, F_g, in the example under discussion becomes 4.05, or more. If, on the other hand, m is equal to or less than g, F_a becomes about - 0.8. Thus the range of F_g (neglecting values of m greater than a and less than g) is shown to be about five times that of F_a, which suggests that a truer comparison of/

of F_g and F_a may be obtained by taking each relative to this 'normal range'. And this, of course, brings us back to our starting-point, i.e. a direct comparison of <u>a</u>, <u>g</u>, and <u>m</u> in terms of simple differences. In Table XXXII

TABLE XXXII.

Stimuli and order.	B F_a	B F_g	B X_a	B X_g	D F_a	D F_g	D X_a	D X_g
- 20: 0 SL	-.34	2.31	17.4	23.1	-.44	1.82	22.3	18.2
- 20: 0 LS	-.68	.59	34.6	5.9	-.74	.32	37.3	3.2
- 40: -20 SL	-.60	1.00	30.5	10.0	-.74	.30	37.5	3.0
- 40: -20 LS	-.74	.32	37.3	3.2	-.65	.76	32.9	7.6
- 60: -40 SL	-.74	.30	37.5	3.0	-.41	2.02	20.3	20.2
- 60: -40 LS	-.71	.47	35.8	4.7	-.74	.30	37.5	3.0

$$X_a = a - m$$
$$\text{and } X_g = m - g$$

and these values are given along with those of F_a and F_g. A comparison of the values may be summarized as follows:

$F_a > F_g$ - 7 times out of 12

$X_a > X_g$ - 11 " " " 12

This second criterion, therefore, definitely favours an approximation of the estimated to the geometric mean. Merkel's criterion ($F_a > F_g$) is less conclusive, but it will be noticed that there is a most frequent/

frequent value of m at about 13. In the data for the ratio 1 : 10 g is favoured in every case by both criteria. (I have not given the detailed results).

The following conclusions may be drawn from these experiments:

(i) The method of mean gradation, using the stimuli and ratios noted, yields values of the estimated mean which are, on the whole, considerably closer to the geometric than to the arithmetic mean.

(ii) Differences of time-order and of general intensity level do not seem to produce any regular variation in the results. The approximation to the geometric mean, however, is closer with terminal stimuli in the ratio 1 : 10 than with those in the ratio 1 : 100.

(iii) A comparison of the results of two observers shows considerable individual differences for each set of standards; these differences, however, show no tendency to occur in any regular manner.

(iv) Introspective evidence from both observers showed that judgements of this sort could be made with a high degree of confidence. The raw data further showed very few inconsistencies in judging the extreme values of the variables, and increased practice yielded increased consistency throughout the range. Both subjects stated that as a rule they judged in terms of sense-distances, i.e., they judged the mid-stimulus as being 'nearer' one or other of the terminal stimuli. This seems to be evidence favouring Delboeuf's 'sense-distance' hypothesis.

(v)/

(v) In general, an approximate though not a full verification of Weber's law for the range studied seems to be indicated.

Estimation of loudness.

A group of experiments on estimation of loudness was devised in an attempt at a direct approach to the relation between stimulus and introspectional or 'intuitive' sensation. Three subjects (A, B, C) took part in these experiments, and at the outset each expressed scepticism as to the possibility of making numerical estimates of loudness.

The general procedure was as follows: A standard intensity was chosen, and to this the value 1 was assigned. This was presented to the subject along with a variable in the form SVSV (i.e. two presentations of the standard along with two of the variable), and the subject was asked to say what fraction or multiple of the standard the variable represented. Each experimental series consisted of 23 - 25 such presentations, and contained only fractional or only multiple values of the variable.

For fractional estimations the standard intensities were - 30 and - 20 db, and for multiple estimations - 30 and - 40 db. In the case of the - 30 db standards 12 variables were used, each being given 10 times, and with the other standards 10 variables, each 5 times. The actual values may be seen in Tables XXXIII - XXXVI. It will be noticed that in the case of the 30 db standards certain values of the variable were omitted. This was done to make the subject's task easier, (see also p.263 below). The following tables contain the mean loudness estimates (L)/

(L) for the three observers, along with the Probable Errors of these mean estimates.

TABLE XXXIII.

Standard - 30 variable -	A		B		C	
	L	P.E.	L	P.E.	L	P.E.
32	.898	.013	.830	.014	.825	.019
34	.670	.027	.710	.029	.700	.034
36	.500	.025	.550	.028	.560	.036
38						
40	.405	.032	.475	.029	.306	.013
42	.215	.022	.385	.023	.313	.015
44	.260	.036	.300	.017	.285	.011
46	.123	.012	.310	.019	.254	.011
48						
50	.108	.0076	.290	.018	.217	.010
52	.078	.0043	.238	.014	.192	.012
54	.063	.0082	.204	.017	.185	.0095
56	.047	.0035	.136	.0096	.166	.0108
58						
60	.041	.0029	.127	.0080	.158	.0037

Smoothed curves showing estimated loudness are given in Figs. 69 - 72. A general comparison of these seems to yield the following results.

Loudness takes a rather different course for each of the observers; in particular A, the least psychologically sophisticated/

sophisticated subject, gave results which differ from those of B and C, which show a closer degree of correspondence with one another. Loudness for A changes with intensity much more rapidly than for B and C - witness the extremely high and low values at the extremes in Figs. 69 and 70. Except for some slight degree of

TABLE XXXIV.

Standard - 30 variable -	A		B		C	
	L	P.E.	L	P.E.	L	P.E.
0	15.7	.569	8.3	.377	7.6	.206
2	12.1	.582	7.0	.472	7.4	.205
4	11.4	.505	4.45	.263	5.6	.249
6	10.9	.546	4.2	.242	5.3	.232
8						
10	7.8	.360	3.05	.166	4.1	.315
12	5.95	.440	2.69	.148	3.52	.216
14	5.8	.474	2.43	.150	2.9	.157
16	5.25	.369	2.15	.165	2.9	.146
18						
20	2.52	.206	1.74	.130	2.05	.120
22	2.37	.205	1.58	.089	1.82	.073
24	1.5	.233	1.28	.063	1.49	.049
26	1.43	.052	1.15	.029	1.53	.058

TABLE XXXV.

Standard - 20 variable -	A		B		C	
	L	P.E.	L	P.E.	L	P.E.
22	.876	.033	.890	.016	.81	.038
24	.540	.060	.760	.013	.65	.032
26	.470	.038	.750	.024	.44	.034
28	.330	.048	.570	.041	.42	.014
30	.250	.045	.480	.033	.28	.0083
32	.133	.021	.284	.025	.26	.013
34	.082	.0062	.280	.031	.26	.016
36	.096	.029	.224	.0076	.22	.0083
38	.066	.0081	.230	.027	.20	.011
40	.074	.0076	.196	.016	.22	.014

TABLE XXXVI.

Standard - 40 variable -	A		B		C	
	L	P.E.	L	P.E.	L	P.E.
20	10.0	.477	3.1	.103	6.0	.477
22	7.2	.416	2.5	.151	4.9	.344
24	5.6	.458	2.06	.079	3.9	.290
26	4.3	.104	2.12	.089	3.2	.165
28	2.9	.197	1.54	.079	2.22	.157
30	2.5	.185	1.44	.050	1.85	.037
32	2.36	.150	1.42	.025	1.72	.048
34	1.82	.054	1.26	.010	1.58	.078
36	1.4	.077	1.19	.055	1.32	.050
38	1.16	.050	1.09	.019	1.54	.017

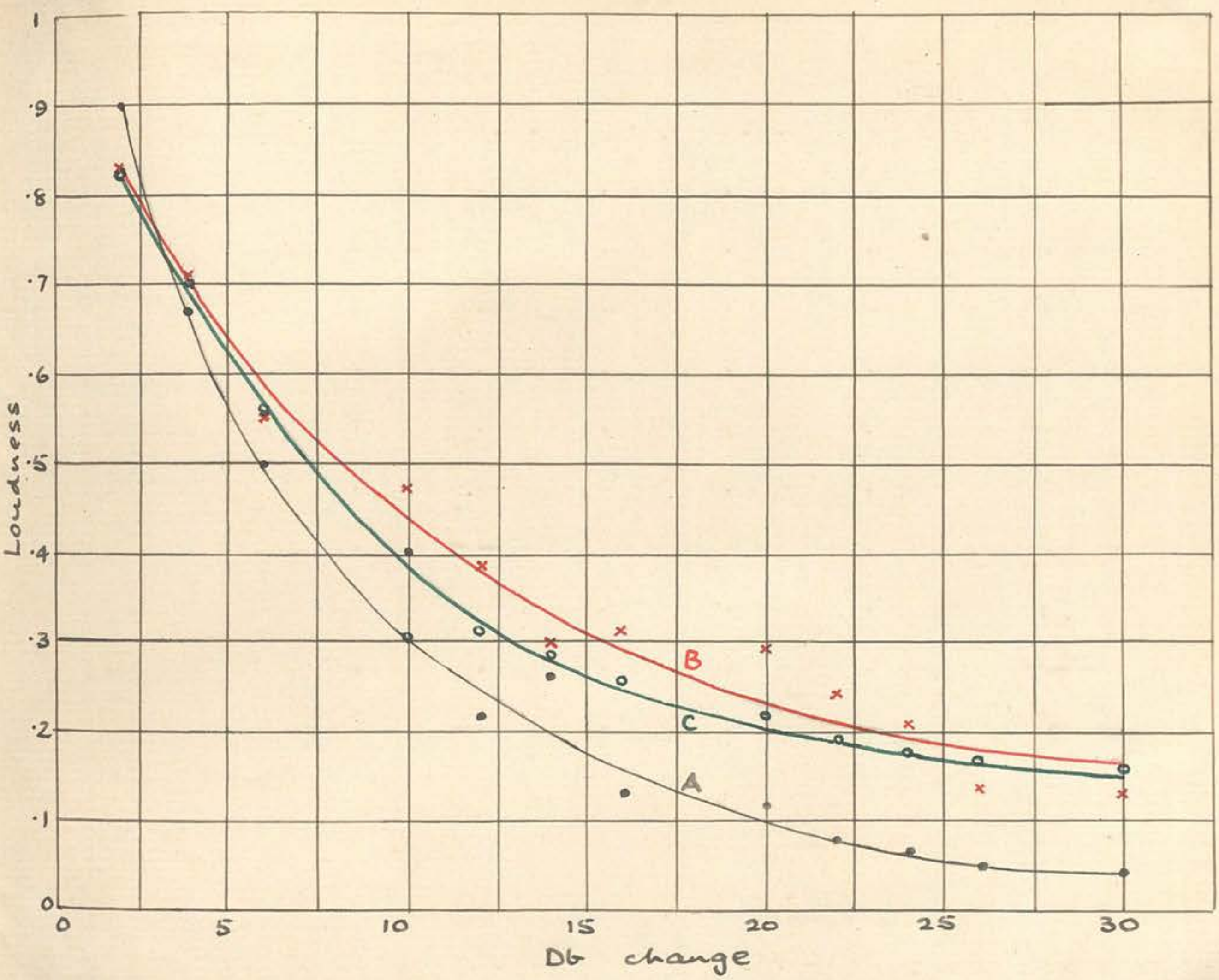


Fig. 69 Fractional loudness - Standard - 30 db

overlap of the curves, which is itself fairly regular, B's curves are consistently further removed from A's than are C's.

I have not attempted to obtain an accurate mathematical analysis of the curves, since the curves required some degree of smoothing, and are therefore somewhat conjectural. It seems fairly certain, however, that a different functional relationship, or at least one with different constants must be found for each observer. Again, it is not possible from these data to deduce a relationship/

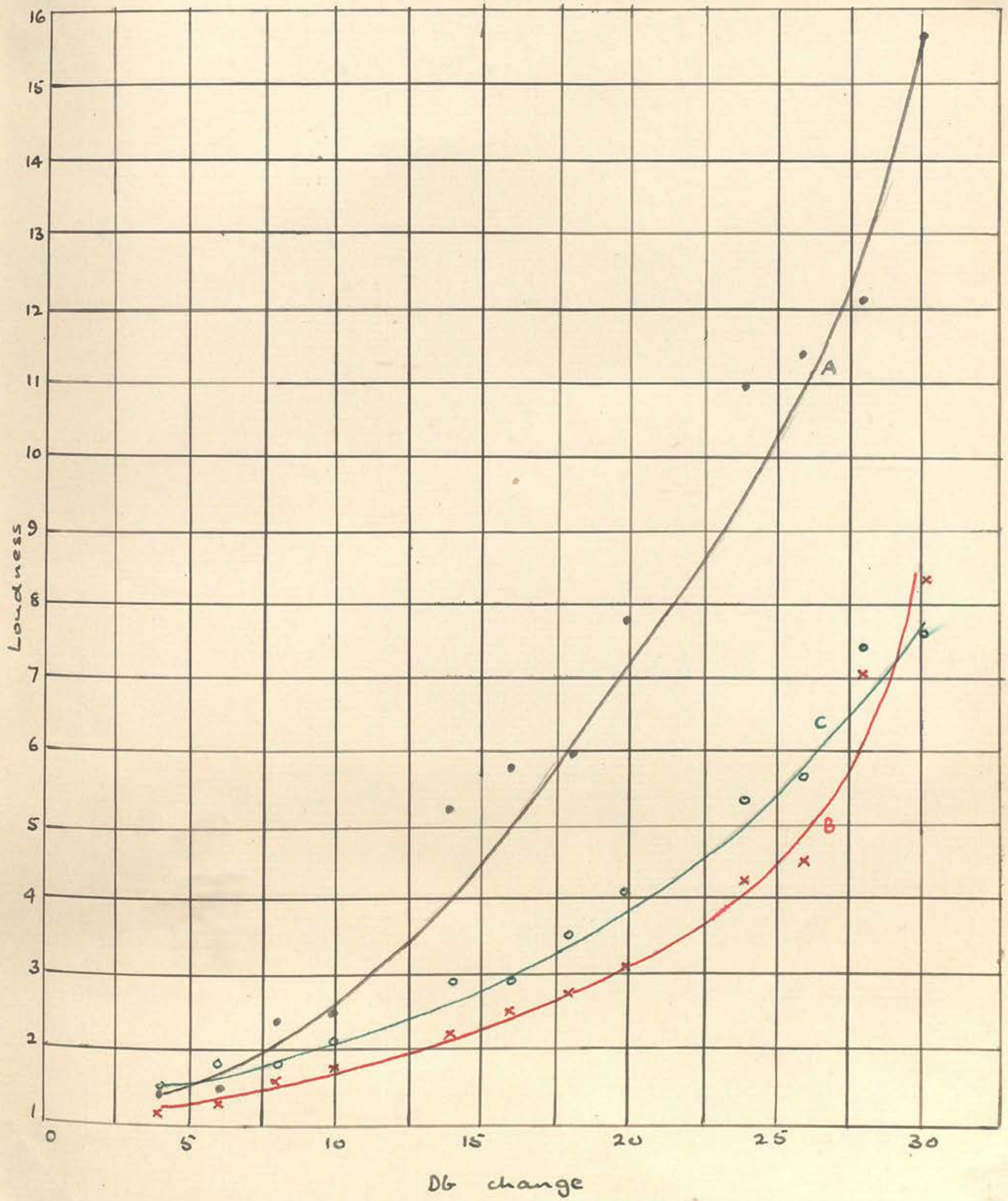


Fig. 70 Multiple loudness : Standard - 30 db

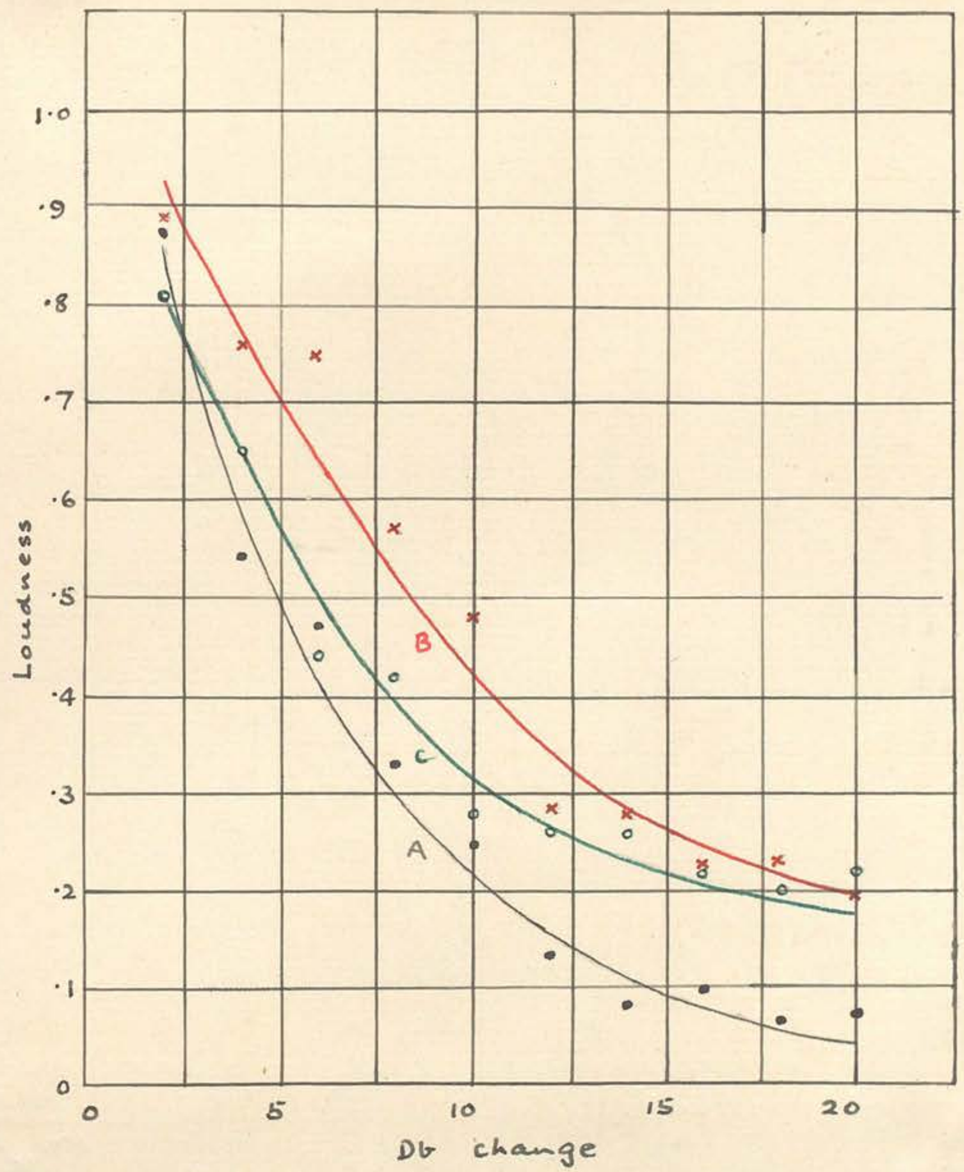


Fig. 71 Fractional loudness: standard - 20 db

relationship between sensation and absolute values of stimulus intensity, since the abscissae of the curves can be interpreted only as db change from an origin represented by the standard intensity. All the curves of Fig. 72 appear to be exponential; that for subject C, in particular, fits the equation

$$L = 0.8 e^{0.1x}$$

remarkably/

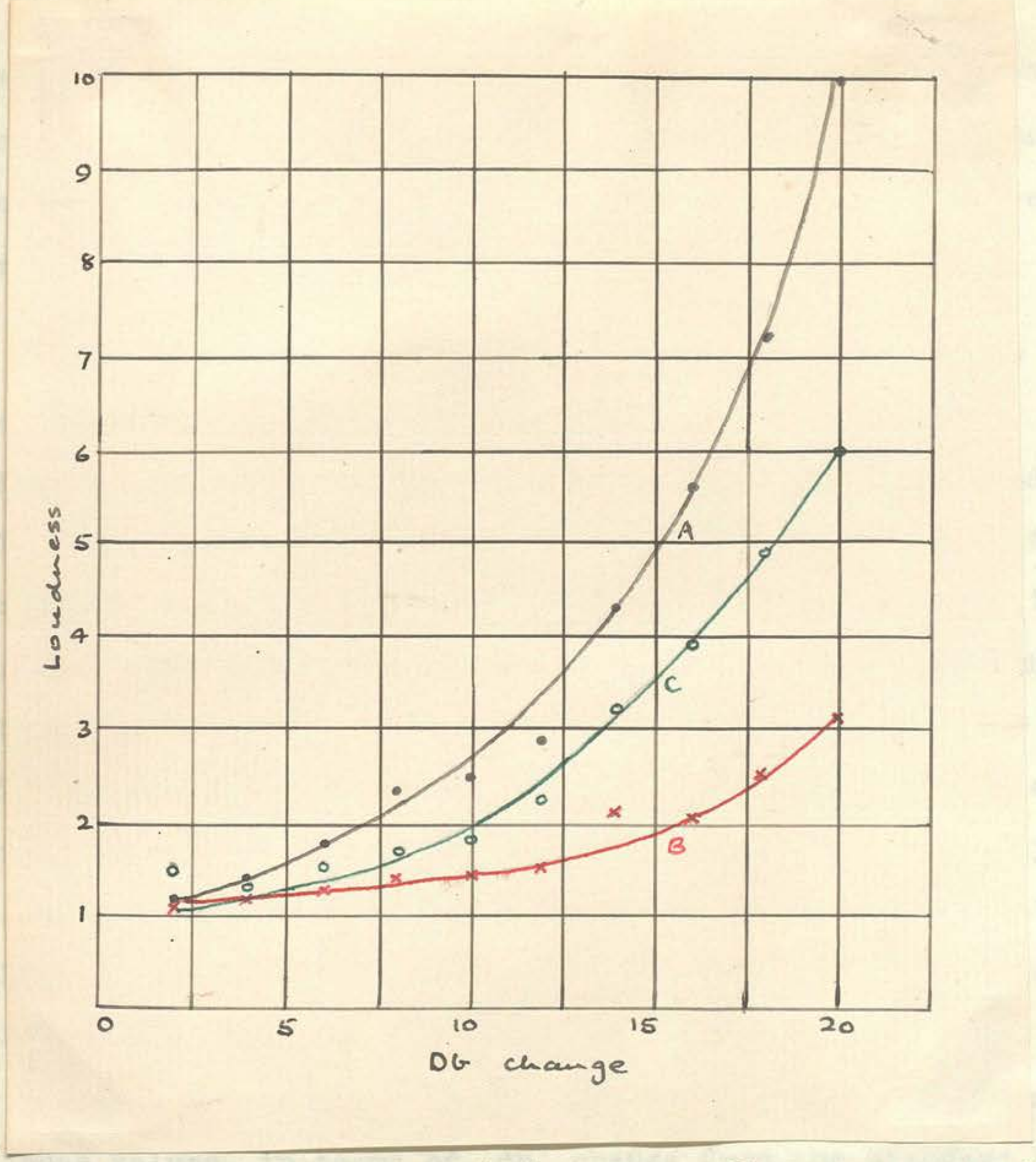


Fig. 72. Multiple loudness: Standard - 40 db

remarkably well. It will be seen that a fair degree of correspondence occurs between Fig. 72 and the first two-thirds of Fig. 70, but that this is not absolute. The same holds true for Figs. 71 and 69. How far the correspondence could have been increased by practice is uncertain.

The exponential relationship does not appear so clearly in Figs. 69 and 71 (fractional loudness), which seem rather to suggest/

suggest a power function. All the subjects reported a considerable change of attitude in passing from one type of estimation to the other. Accordingly, it is quite conceivable that different laws may cover the two cases.

The numerical estimates were checked to some degree by a method similar to that used by Laird, Taylor and Wille (26) Each of the subjects was required to judge for 'half' and 'double' loudness by the method of Right and Wrong cases. A suitable range of variables was chosen from the estimation data; i.e., a series was formed containing all or most of the values which had been judged as .5 or 2 times the standard, as the case might be. These were presented to the subject along with the standard in random order (in the form SVSV as before), and the subject was asked to judge each as 'greater than', 'equal to', or 'less than half (or double)' the standard. The results were treated in the same way as those of the method of mean gradation, i.e., a 50% point was interpolated in the 'greater + ½ equal' frequency column. These values, in terms of db change from the standard intensity are given in Table XXXVII, (marked J).

TABLE XXXVII.

Standard and Method	Half.			Double.		
	A	B	C	A	B	C
20 E	5.0	8.5	6.0			
20 J	4.3	8.3	6.0			
30 E	6.0	8.5	7.5	7.5	13.0	10.0
30 J	4.8	7.3	8.0	6.5	10.0	13.7
40 E				7.0	15.5	10
40 J				-	9	12.8

The number of presentations was not extended to any great length (5 of each variable), partly because the subjects found this task extremely boring, and partly because they judged with a very high degree of consistency. On the other hand, the degree of agreement with the results of estimation is far from absolute. These latter values (marked E) are also given in Table XXXVII, and are obtained from the smoothed curves of Figs. 69 - 72. It will be seen that the agreement is greater for half than for double loudness. In all cases, however, the relatively different 'characteristics' of A, B, and C are brought out; the biggest discrepancy is in respect of reversals ~~too~~ between B and C in the case of double loudness, by the judgement method.

Certain changes of mental standard inevitably occurred in the course of this experiment, but the subject usually quickly fell in with his previous scale of estimation. Probably a practice period should be given at the beginning of every sitting in a case like this. Practice seems to be largely a matter of attention, and it seems unlikely that actual discrimination in audition in general is affected by practice. This point is brought out by Vernon (262), who shows that since it is supplied with afferent nerves, the cochlear cannot be much affected by training.

It seems on the whole that measurement by this means is possible, and I do not think that the practice, reported by C and to a less degree by A, of identifying a given intensity and assigning an arbitrary numberical value to it, is a serious hindrance to the method. This identification was sometimes, but not always, facilitated by omitting certain values in the case of the - 30 db standards.

SUMMARY AND CONCLUSIONS.

The following summary recapitulates the main points put forward in my review of Weber's law applied to the intensity of sound and the accompanying discussion. The topics dealt with are numbered to correspond with the division into sections. Certain general conclusions, not previously made, are also included here.

I. Weber's law, or the Weber-Fechner law, if the name is to survive at all, must be interpreted on a psychological basis. That is to say, it must deal in some way with events of consciousness, since Weber's original statement dealt with the act of comparison, and Fechner's interest was in the relation between objective and subjective phenomena. Johnson's objection (152) that Fechner's formulation involved an undefined variable (sensation) is, on this view, invalid, since common experiences recognizes the real occurrence of sensation: as Boring (77) puts it, 'Everybody known what γ (Fechner's S) is, independently of β (Fechner's R)', whereas what β itself is cannot be said with certainty. It is therefore more logical to define stimulus in terms of sensation than sensation in terms of stimulus. A psycho-physical interpretation must further be ruled out, since practically no-one now believes (though Kümmel (164) is a comparatively recent exception) that every smallest alteration of stimulus produces a sensation change.

The possibility of a logarithmic relation between S and R , in/

in whatever sense they may be interpreted, stands or falls not so much by the equality of just noticeable differences, but by the measurability of sensation. Although sensation is certainly not measurable in any sort of objective units, the general opinion seems now to be that in some sense or other sensation is measurable. This seems to have been the conclusion drawn at the Leicester meeting of the British Association (1932). In the symposium already quoted, Houston (149) stated that he personally was certain that sensation could be measured. The view that sensory discrimination is based on the occurrence or absence of awareness of sameness or difference, and that judgements of sameness or difference may be treated by the Law of Error was represented at that symposium by Bartlett (60). A synthesis of this view with a recognition of the effects of absolute 'bigness' and 'littleness' characterizing sensory events, would seem to afford a pragmatic justification of the measurement of sensation. The results of such measurement, if subjected to mathematical analysis, may or may not be found to take the form of a logarithmic equation.

II. In the investigation of sensibility to intensity differences of sound many difficulties due to the physical properties of sound waves, and to peculiarities of the hearing mechanism, are encountered. Divergent results may be due to a variety of factors very hard to control; Sivian and White (229) call attention in particular to the possibility of effects of wave motion in the meatus, and of diffraction caused by the listener's head.

The best results are probably to be obtained by using some apparatus which converts acoustical energy into equivalent units of/

of electric current. In any case it is advisable to use the decibel scale, since the response of the ear is at all events approximately logarithmic, so that simple units of pressure or energy flow soon reach unmanageable proportions.

other

III. Certain psychological properties of sound must also be considered, as well as certain phenomena which may be said to be of

between the physical & psychological

an intermediate nature. In dealing with any sound other than a pure tone, masking phenomena must be recognized, and compensated for if absolute measurement by summation is required. These phenomena, as Wegel and Lane (272) point out, are largely responsible for the non-occurrence of a linear relation at high intensities between sound pressure and response of the ear.

In addition, sensory discrimination is affected by a large number of purely subjective factors, such as those listed by Fernberger (114) as governing equality judgements, e.g., attention, instructions, comprehension, temperament, etc. Some of the phenomena of hearing seem to have withstood all attempts at explanation in physical terms; on the other hand, one cannot agree with Watt(269), who stated that all sensory experiences can be accounted for in systematic terms without recourse to the discoveries of physics or physiology.

IV. A summary of work on auditory intensive thresholds has already been given (pp. 152-4). At best, $\Delta R/R$ for sound intensity is constant only for a limited range.

V./

V. Direct estimation or judgement of loudness in absolute units provides rather inconclusive results. Constancy is usually obtained within the bounds of an individual investigation, even among the different subjects participating in that investigation. On the other hand, agreement between investigators is rather exceptional, except in very general terms. Analysis of results, however, seems now to favour an exponential equation relating loudness to intensity of sound measured in db. The decibel scale, though useful in its own way is very misleading if interpreted as giving numerical indications of loudness.

Measurement may also be accomplished by a method of balancing loudness, preferably against a definite reference tone. This is most difficult at the extremes of the frequency range, where the number of distinguishable intensities is small, and consequently, as Tucker (257) points out, it may not be possible to match with the reference tone.

VI. Many writers have reduced the status of Weber's law to that of a special case of a generalized relativity law. Various attempts have also been made to determine a more accurate mathematical formulation representing sensory discrimination in the different fields. Riesz (36) showed that his data on sound intensity were satisfied/

satisfied by the equation

$$\frac{\Delta J}{J} = S_{\infty} + (S_t - S_{\infty})\, 10^{\frac{-\alpha_s^{\,n}}{10}}$$

where $S_{\infty} = .000015\, f + \dfrac{126}{80\, f^{0.5} + f}$

$$S_t = .3 + .0003\, f \quad \frac{193}{f^{0.8}}$$

$$n = \frac{24,400}{358,000 f^{0.125} + f2} + \frac{.65\, f}{3500 + f}.$$

In this equation S represents differential sensitivity for high intensities, and S_t sensitivity at the absolute threshold, (α_s); f is frequency. This result is quoted by Fletcher (8, 9) as authoritative, but it seems to me that the interest is limited, since differential sensitivity varies greatly with the individual as well as with the type of stimulus.

On the whole it is best to take Weber's law from the phenomenological point of view - i.e., as an approximation best describing the general form of a number of observed facts. Thus, we may, for example follow Lloyd Morgan (181), who proposed the modification: 'For constant increments of sensation, the concomitant increments of stimulus are in geometrical progression', introducing the qualification 'approximately'. Better still, it is possible to eliminate intensity from the problem altogether, and consider Weber's law as expressing a relation between different modes of consciousness (cf. Gatti (127), Cobb (98)).

The problem has also been approached from a physiological angle; in this case sound intensity discrimination is bound up with the theory of hearing. Since it is not possible to work with living/

living human ears, experimental work has been] confined to work with animals, and with models of an 'artificial ear', (e.g. by Békésy (63)), and Langenheck (167) who showed the limitations imposed upon this method by the use of imperfect materials.). The results of recent work seem to indicate that some theory based on the all-or-none principle must hold good, but work on the different sense-departments still awaits synthesis. The recent discoveries alluded to have rendered many standard and semi-popular books (e.g. that of Ogden (196)) out-of-date, and a complete survey of recent work on hearing would no doubt prove valuable.

VII. The practical implications of the difference threshold are numerous, and in many cases too obvious to attract attention. A case in point is comprehension of speech, discussed by Marx (30).

VIII. The original experiments here described lead to the following conclusions:

(i) Weber's law holds very approximately for a limited range of sound intensities.

(ii) The deviations are of a continuous nature. This bears out the findings of Kenneth and Thouless (23), and Riesz (36). Upper deviations seem to have been conclusively demonstrated.

(iii) Individual differences are very noticeable, and often even surprising (cf. Weiss (274)), and day-to-day variations for the same observer may also occur.

(iv) All the subjects tended, to quote a phrase used by Banister/

Banister (56), to objectify their experience to a high degree. Thus in the watch-tick experiment, the criterion was often intuited nearness rather than apparent loudness. In the phonometer experiment, again, one subject (B) found it helpful to think of the sounds as produced by hammer-blows of varying force; another subject (C) equated the tuning-fork tones with the energy he would have required to sing them.

(v) Different statistical procedures yield widely different thresholds. It is doubtful whether the labour of the Constant method ever justifies results. Interpolation, as advocated by Newhall (191, 192) is probably sufficient for most purposes. Thresholds based on Spearman's (295) or Wirth's (see 258) formulae seem to yield too low values.

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